BULLETIN OF THE HISTORY OF MEDICINE

ORGAN OF
THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
AND OF
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

Founded by HENRY E. SIGERIST

Editor—OWSEI TEMKIN

Assistant Editor—C. LILIAN TEMKIN

ADVISORY EDITORIAL BOARD

WYNDHAM B. BLANTON
GEORGE W. CORNER
ESMOND R. LONG
A. EARL WALKER

VOLUME XXXIII

BALTIMORE
THE JOHNS HOPKINS PRESS
1959

Copyright, 1959 by The Johns Hopkins Press

TABLE OF CONTENTS

MAIN ARTICLES	PAGE
Hideyo Noguchi, 1876-1928: PAUL FRANKLIN CLARK	1
Harvey in London: EDGAR F. MAUER	21
The Cholera Epidemic of 1832 in New York City: CHARLES E. ROSENBERG	37
Palaeopathology of a California Archaeological Site: JAMES G. RONEY, JR.	97
When Medicine was in Flower: LYNN THORNDIKE	110
Osborne Reynolds of Manchester, Contributions of an Engineer to the Understanding of Cardiovascular Sound: VICTOR A. MCKUSICK and H. KENNETH WISKIND	116
A History of the Western Dental College: RALPH W. EDWARDS	137
Walter Reed, C. Finlay, and their Predecessors around 1800: SIGISMUND PELLER	195
Erasistratus, Galen, and the *Pneuma*: LEONARD G. WILSON	293
Japan's Encounter with Western Medical Science: "The Beginning of Dutch Study," Being the Memoirs of an 18th Century Doctor: EIKOH MA	315
Medical Men and the Beginnings of Veterinary Medicine in America: J. F. SMITHCORS	330
John Browne, 1642-1702, A Seventeenth Century Surgeon, Anatomist, and Plagiarist: K. F. RUSSELL	393, 503
Johann Jakob Guggenbühl and the Abendberg: LEO KANNER	489

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE: Thirty-First Annual Meeting (Cont'd.):

The Dilemma of Johann Jacob Wepfer: The History of the Contracted Kidney: PAUL KLEMPERER and WALTER FEDERN	50
The National Institutes of Health Research Grant Program and the History and Sociocultural Aspects of Medicine: PHILIP SAPIR and JEANNE BRAND	67

PAGE

Theories of Vision and Color Perception of Empedocles and Democritus; Some Similarities to the Modern Approach: Rudolph E. Siegel 145
The Changing Ideology of Health Museums and Health Fairs Since 1850: Bruno Gebhard 160
Harvey and the Circulation of the Blood in America During the Colonial Period: F. Guerra 212
Pietro Pisani and the Mental Hospital of Palermo in the Early 19th Century: George Mora 230
The Development of Physiology in the Last Fifty Years: Chandler McC. Brooks 249
Microscopic Physiology Since 1908: Paul F. Cranefield .. 263

American Association for the History of Medicine: Thirty-Second Annual Meeting:

Some Hoaxes in the Medical Literature: Saul Jarcho 342
Report of the Thirty-Second Annual Meeting, Cleveland, Ohio, May 21, 22, and 23, 1959 348
The Fielding H. Garrison Lecture: A View of 19th Century Medical Historiography in the United States of America: W. B. McDaniel, 2d 415
The Inception of Anatomical Studies in the University of Paris: C. D. O'Malley 436
Auscultation in England, 1821-1837: Lester S. King 446
Social Class and Psychological Medicine in the United States, 1789-1824: Norman Dain and Eric T. Carlson 454
Neural Pathology: A Pathogenetic Concept of the 18th and 19th Centuries: Gernot Rath 526

Texts and Documents

An Essay on Coughs by Locke and Sydenham: Kenneth Dewhurst 366

Notes and Comments

Postscriptum on John Morgan and Giovan Battista Morgagni: Antonio Pace 75
A Sixteenth Century Tracheostomy?: C. A. Holborow 168

PAGE

Comments on Dr. Romanell's Article on Locke and Sydenham: David L. Cowen 173

A Classic of Microscopy: An Early, if not the First, Observation on the Fluidity of the Axoplasm, Micromanipulation, and the Use of the Coverslip: Hebbel E. Hoff 375

Jan Stephan van Calcar: A Little Known Self-Portrait: Robert Proper 466

A Review of Auenbrugger's *Inventum Novum*, Attributed to Oliver Goldsmith: Saul Jarcho 470

Bibliographies

Bibliography of the History of Medicine of the United States and Canada—1958: Genevieve Miller 542

The Johns Hopkins Institute of the History of Medicine:

Report of Its Activities During the Academic Year 1958-59: Owsei Temkin 574

Training Program in the History of Medicine 579

Medico-Historical News and Activities

Correspondence and Reports

Sixteenth International Congress of the History of Medicine (George W. Corner) 78

Historical Medical Museum in Tel Aviv (Joshua O. Leibowitz) 80

Bertha Spector (Owsei Temkin and C. Lilian Temkin) 80

National Institutes of Health Research Grant Awards in the History of Medicine 380, 476

Olof T. Hult, 1868-1958 (Anne Tjomsland) 475

Faculty of the History of Medicine and Pharmacy, of the Worshipful Society of Apothecaries of London 581

Announcements 81, 181, 276, 382, 477, 583

Book Reviews 84, 184, 279, 385, 480, 586

Contributors to this Volume 95, 194, 291, 392, 488, 588

Index of Names 589

Hideyo Noguchi

Noguchi, about 43 years old. Photograph by Louis Schmidt at the Rockefeller Institute about 1920.

BULLETIN OF THE HISTORY OF MEDICINE

ORGAN OF
THE AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE
AND OF
THE JOHNS HOPKINS INSTITUTE OF THE HISTORY OF MEDICINE

VOLUME XXXIII JANUARY–FEBRUARY 1959 NUMBER 1

HIDEYO NOGUCHI, 1876–1928 *

PAUL FRANKLIN CLARK

Preface

Hideyo Noguchi, 1876-1928, was one of the most productive, as well as one of the most controversial figures in American bacteriology during the first quarter of the twentieth century. We have the several biographical sketches published shortly after his tragic premature death in Africa from yellow fever. In addition to the usual "*de mortuis nil nisi bonum*" attitudes, these papers are especially colored with emotion because Noguchi died while endeavoring to determine the truth or falsity of his findings in yellow fever in this hemisphere. We also have the vivid biography by Gustav Eckstein [1] published in 1931; Eckstein never knew Noguchi but he did a masterly job of portraying the character of the man, his background, and his emotional reactions. The brief snappy chapters, the staccato sentences, frequently with no formal structure, the letters he obtained in Japan, seem to breathe Noguchi. Although many of Noguchi's findings had been seriously questioned by the time of his death, facts dug out since that time, as well as friendly scar tissue over the wounds, make it easier for us to paint not a more accurate portrait of the

* Prepared for the William Snow Miller Medical History Seminar at the University of Wisconsin, February 12, 1958.

[1] Gustav Eckstein: *Noguchi*. New York: Harper & Brothers, 1931. Extracts from Noguchi's letters are culled from this vivid biography. Permission by author and publisher to quote from this biography is gratefully acknowledged.

man, but a clearer estimate of his scientific accomplishments. I knew Noguchi rather well although never intimately. By example he taught me some of the tricks of serology, especially complement fixation; his laboratory and mine were on the same floor at the Rockefeller Institute, and in those days, the group in the lunch room was small, providing easy friendliness. Throughout his life, as I revisited the Institute, he was generous in providing material from his current investigations for me to study. I was fond of Noguchi; we all were. We appreciated him, his direct childlike simplicity and this intense vigor, in spite of, possibly in part because of, his weaknesses and contrasting moods.

Noguchi's early life was hard. Born Seisaku Noguchi in extreme poverty in a tiny Japanese village, his left hand burned badly by a fall into the fire at the age of two, his education and advancement were achieved through the great self-sacrifice of his mother and sister, his own hard work, and the convincing excitement and enthusiasm which constantly persuaded friends and strangers that he was worth helping and worth rescuing, even when he had foolishly squandered the money they had squeezed out to give him. Even years later, at the Rockefeller Institute with a liberal salary, he was always borrowing on next month's check. His letters to his friends were commonly begging, but always expressed in picturesque imagery. He was ever the artist, and even his change of name reflects his sense of his own destiny: " Hideyo," great-man-of-the-world, adopted when he was feeling particularly despondent, uplifts him and sets his goal anew.

Noguchi at the University of Pennsylvania

Several Americans, among them Simon Flexner,[2] professor of pathology at the University of Pennsylvania, went to Tokyo in 1899 on their way to the Philippines to investigate dysentery among the soldiers. During their visit to the Kitasato Institute of Infectious Diseases, where he was working, Noguchi said he would like to come to America. Flexner, treating this as a casual remark, says with equal casualness, " That would be fine," and thinks nothing more of the incident. To Noguchi, however, this is the open sesame for which he has been longing. His departure delayed by his usual improvidence, and steerage passage to San Francisco finally ordered for him by a friend, Noguchi began his American life in Philadelphia at the end of 1899 by going at once to see " The Professor,"

[2] Simon Flexner: Hideyo Noguchi. *Science*, 1929, *69*: 653-660.

Dr. Flexner. Several times, I have heard Dr. Flexner tell with great relish how Noguchi appeared out of the blue, laden with gifts in good oriental fashion and wreathed in smiles. Poor Noguchi's smiles were soon wiped out. How little he realized that more than a simple letter to the Professor was essential to assure him a position in a strange land. But Dr. Flexner bustled around, and as usual, a friend came to Noguchi's rescue, in this case Dr. S. Weir Mitchell[3] who, with his father before him, and with E. T. Reichert,[4] had studied the action of snake venoms. They had shown that snake venoms are proteins and that some contain toxic albumins, and had demonstrated experimentally some of the poisonous effects of snake venoms on nerve tissue and on blood. Mitchell wondered if the current knowledge of immunity and antitoxins might provide a new method for the study of these complex mysterious protein substances. So he provided the money, Flexner and the University of Pennsylvania provided equipment and space, and Noguchi provided the energy.

The Philadelphia period was for Noguchi a time of difficult adjustment to American laboratories and American life. His English was atrocious, his knowledge of venoms and immunologic techniques practically nil. Fortunately four young colleagues, Henry Bunting,[5] Fred Gay, Richard Pearce, and Jack Yates, none better, were continuing their medical training under Dr. Flexner, and they gladly took over Noguchi's day by day education. His life in the laboratory was sometimes happy, sometimes painful, but always puzzling. Noguchi beamed when Yates termed him " the yellow peril." Limericks became important in his learning colloquial English. He would cock his massive head with its mop of curly black hair on top of his small body (about five feet one inch in height), look up at the speaker with a blank expression on his face and a serious question in his eye. Then when he began to realize that the apparently serious statement was just an American joke, his eyes would light up and a delighted broad grin would break out whether he understood the joke or not.[6] That he was learning was apparent when one day he put a sign on some of his experiments, *No-touchi* / *No-guchi.* Or again, when they found him

[3] S. Weir Mitchell: *Researches upon the Venom of the Rattlesnake*: with an investigation of the anatomy and physiology of the organs concerned [1860]. Washington: Smithsonian Institution, 1861.

[4] S. Weir Mitchell and E. T. Reichert: *Researches upon the venom of poisonous serpents.* Washington: Smithsonian Institution, 1886.

[5] Personal recollections supplied by Charles Henry Bunting.

[6] Personal recollections of the author.

carefully treasuring an old male rat, the progenitor of the laboratory colony, in his laboratory coat pocket for days, a question " why " brought forth the grinning response " pater emeritus." He sparkled when at Yates' suggestion a new organization was formed, and Noguchi was formally vested with all the rights, dignities, and privileges of charter membership in the *Society for the Liberation of Captive Balloons.*[7] Noguchi liberated his full share.

On the venoms, Noguchi was working more consistently than ever before. When experiments were successful, he was sure he had the world by the tail; after a series of failures or negative results, he was in the seventh hell. He refused an invitation to an evening ball game with " Too much to do. Today, library, tomorrow, interesting experiments." Adverse criticism he took badly. " Dr. Flexner very hard on me; I go home and I weeps and I weeps." Sensitive, likeable, generous to a fault save where his scientific honor was concerned, he never had intimates among his American colleagues; that relation was reserved for his countrymen.

His method is multiple experiments now and always. He published several papers with Flexner so that he wrote gaily to his Japanese friends that the venom problems were largely solved. " I am finishing with great success." The Carnegie Institution is willing to subsidize a monograph on snake venoms, and Noguchi thinks that will be accomplished shortly. Actually that distinguished publication with many drawings by Noguchi did not come out until 1909.[8]

Many folklore tales and innumerable studies have been stimulated by the tragic drama of sudden death following the bite of venomous animals. The first methodical study was by the Italian scientist, Redi, published in 1664, and a century later another by Fontana, in 1767.[9] In this country the studies by Noguchi's fostering friend, Mitchell,[10] and Mitchell and Reichert,[11] were probably more important than those of Flexner and Noguchi. The detailed descriptions of hemolysis produced by venoms and of the specific damage to the endothelium of blood vessels resulting in edema and hemorrhage were, I believe, the more significant contributions of Flexner and Noguchi. Calmette's studies, begun in 1891 at the newly

[7] Bunting, personal recollections.

[8] Hideyo Noguchi: *Snake Venoms.* Carnegie Institution of Washington, 1909.

[9] Chauncey D. Leake: Development of knowledge about venoms. In *Venoms,* Papers presented at the first international conference on venoms, 1954. Washington: American Association for the Advancement of Science, 1956.

[10] Mitchell, *op. cit.*

[11] Mitchell and Reichert, *op. cit.*

created Pasteur Institute at Saigon and continued for years in Paris with several associates, are pre-eminent in this field. In his monograph, Calmette [12] expressed appreciation of the well organized work of Flexner and Noguchi, but he gives credit to Henry Sewall,[13] at that time professor of physiology at the University of Michigan, for the first (1887) preventive inoculation against snake venom following repeated injections of sublethal doses of rattlesnake venom into pigeons.

Work in Copenhagen

On the basis of the venom studies, Noguchi received, in 1903, a fellowship for study with Madsen in Copenhagen. This was not the opportunity in Germany of which he had been dreaming, but Denmark was close to Germany. Who could predict? Perhaps!! The experience with Madsen proved highly enriching for Noguchi. He enjoyed the small laboratory with its friendly atmosphere and systematic regime. He learned some exact methods from Madsen, although he never became an orderly worker. He and Madsen produced an antivenin in goats by the injection of repeated doses of reconstituted dried rattlesnake venom. He says that he has never been so happy or had it so good in his life as now.

In the midst of all this exciting life in Denmark, Japan went to war with Russia. Naturally Noguchi was greatly disturbed; he felt his isolation keenly, the lack of direct news from Japan, the strong positions taken by the newspapers, the exaggerated statements, many of them quite antagonistic to Japan. But the naval victory at Port Arthur placed Admiral Togo high on the pages of history, changed many of the attitudes towards Japan, and restored Noguchi's confidence.

Rockefeller Institute

In 1901, the Rockefeller Institute for Medical Research had been founded through the generous philanthropy of John D. Rockefeller, Sr., and the vision of his adviser, Frederick T. Gates. At first some grants-in-aid, then a rented building, and in 1906, the first laboratory building, now known as Founders Hall, was opened with Simon Flexner from the University of Pennsylvania as director, and William Welch of Johns Hopkins Medical School as president of the board of scientific directors.

[12] A. Calmette: *Venoms*, translated by E. E. Austen. New York: Wood & Co., 1908.
[13] Henry Sewall: Experiments on preventive inoculation of rattlesnake venom. *J. Physiol.*, 1887, *8*: 203-210.

Back in this country from Denmark, Noguchi was uplifted by the enlarged opportunities the Institute provided for him. He wrote his benefactor, Chiwaki,[14] " I am first assistant which corresponds to head of department and am studying hard." " Flexner is head. There will be six including me, of whom two are professors, and they are thirty years up in age and they are above me, but their departments are different. The remaining three are below me, and if I say from age, I am the youngest, because the rest are above thirty, with wives. So the future is bright if it goes as now."

We may properly assume that Noguchi had by this time completed his training and that we, his observers and friendly critics, now know and appreciate much of his background and his character. From this time until his death he worked frequently with a fierce intensity on etiological problems of so many different diseases that it makes one dizzy just to list them. He wrote again, " My research is still snake venom and I will continue till the book is finished." And to Madsen, apologizing for his failure to write, he says, " Besides, I was working on trachoma, on *B. tetani* . . . and I have made observations concerning the photodynamic action of certain fluorescent substances upon snake venom, tetanolysin and tetanospasmin." In assaying his accomplishments we must bear in mind that although he always had the remarkably fine *diener* and other services of the Institute and a few trained assistants, he never had anything approaching the large organizations we are familiar with today. And always everything passed through his hands and each slide under his microscope.

To give a glimpse of the overwhelming variety of fields in which Noguchi delved, I shall list the important ones and the number of his published contributions in each.

Snake venoms and other exotoxins	21	and an extensive monograph
Antibodies	12	
Complement and complement fixation	21	
Spirochaetes and their cultivation	31	
Tuberculosis	2	
Sporulation of bacteria	2	
Syphilis and serum diagnosis of this disease	27	and a book that went through several editions
Treponema pallidum in general paresis and tabes dorsalis	9	

[14] Eckstein, *op. cit.*, p. 136.

Trachoma	7
Poliomyelitis	5
Rabies	4
Vaccines	4
Rocky Mountain spotted fever	5
Oroya fever and verruga peruviana	20
Yellow fever	34
Miscellaneous	7
	212

Noguchi's papers were commonly well edited, possibly too much so. Gradually he obtained excellent command of written English but was never fluent with the spoken word. To get a notion of the working of his mind, his letters, liberally cited by his biographer, Eckstein, are always a better source.

Spirochaetes

In 1905, came the exciting news from Germany that Schaudinn, a young protozoologist, had discovered the causative agent of syphilis, a spirochaete, *Treponema pallidum*. This finding was promptly confirmed and, with Wassermann's application of Bordet's complement fixation for diagnosis of syphilis, the world, including Noguchi, was fired with the possibility of the control of this devastating venereal disease. Noguchi worked late into the night studying each of the components in this reaction and developed a modification in which the sheep corpuscles were replaced by human corpuscles and the antisheep system by an antihuman hemolytic system. This method avoided a number of the difficulties of Wassermann's procedures, especially the presence of antisheep hemolysins in some human sera. It was used somewhat in a few hospitals but never achieved any widespread success. Shortly there were a dozen modifications of the original method. Every doctor wanted to know about the diagnosis of syphilis, and Noguchi, at fever heat, wrote a helpful book on the *Serum Diagnosis of Syphilis*, a chapter a night with some aid from a colleague, Dr. Kaliski, but more from many large black cigars, preferably a favorite fifty cent brand, a little whiskey, and a pot of tea always available.

The more permanent value of his serum diagnosis work was that it led him to the cultivation of spirochaetes. A number of investigators had grown some of the less parasitic spirochaetes in vitro, and Schereschewsky[18] had reported the cultivation of *T. Pallidum*, though not in

[18] J. Schereschewsky: Züchtung der Spirochaeta pallida. *Deutsche med. Wchnschr.*, 1909, *35*: 835, 1260, 1652.

pure culture. After irregular success with some of the more easily cultivable organisms, Noguchi adopted a suggestion of Theobald Smith on the use of bits of sterile tissue, preferably rabbit kidney, in tall narrow tubes containing serum-water with a layer of paraffin oil on top to aid in providing varying degrees of anaerobiasis. Instead of depending on an irregular supply of spirochaetes directly from human sources, Noguchi successfully produced lesions in the testicles of rabbits from which he could secure a more dependable supply. Noguchi's attack on this problem was so highly characteristic that a few details now will serve as a description of his investigations in other fields. He rails at the suggestion of genius. " There is no such thing as genius . . . to work three, four, five times harder than anyone else, that is genius." And that is the way he tackled the cultivation of *Treponema pallidum*, two hundred tubes to the other man's ten. And he examined each tube himself far into the night, or all night, if that became necessary to finish the job. And then Dr. Flexner would scold him for not being in his laboratory when he " made rounds " about ten or ten-thirty the next morning.

Noguchi's personal life continued in America to have many episodes similar to those of his earlier days in Japan. His intimate friends were Japanese, and frequently one or another shared his apartment. When he was not working late at the laboratory, he might be writing furiously at home with a pot of tea at hand, or he might be taking an old Tokyo friend to see the town, oblivious both of time and money. He loved to play chess. Differing from most devotees of that game, he made fast moves, smoked incessantly while he played many games in an evening. He was eager to win but wanted a contestant to make the winning an achievement.

Meanwhile the work with the spirochaetes went on intensively. In 1911, Noguchi stated, " This is the first time that *Treponema pallidum* has been proven, beyond all doubt, to have been obtained in pure culture," [10] and about a year later he reported the production of " specific " skin lesions in monkeys and in chimpanzees following the injection of his cultivated organisms. With three different strains cultivated directly from human sources (number of transplants from original source is not stated), he produced skin lesions in two species of monkeys " resembling the primary syphilis in man." As the lesions progressed, the monkeys developed

[10] Hideyo Noguchi: A method for the pure cultivation of pathogenic Treponema pallidum. *J. Exper. Med.*, 1911, *14*: 99-108. Also, Noguchi: The direct cultivation of Treponema pallidum pathogenic for the monkey. *J. Exper. Med.*, 1912, *15*: 90-100.

positive blood Wassermann reactions. Other investigators have been unable to duplicate Noguchi's results. Even Evelyn Tilden [17] who worked in this field with him at the Institute was unable to confirm the results when she went later to Northwestern Medical School. Whether Noguchi successfully cultivated virulent *T. pallidum* remains therefore a moot question. Personally I thought his demonstration adequate but I was, I fear, always somewhat biased in Noguchi's favor. We would all agree with Noguchi that sera and ascitic fluids from different sources vary greatly in their capacity to support the growth of highly parasitic organisms.

The luetin skin test which Noguchi developed, using masses of his cultivated *T. pallidum*, nas proved untrustworthy. However, he became a master of the field of spirochaetes, and his monograph on that subject, not published until later as a chapter [18] in Jordan and Falk's *Newer Knowledge of Bacteriology* (1928), is excellent and is beautifully illustrated by Noguchi's own drawings.

The long controversy over the etiology of general paresis and tabes dorsalis [19] was settled by Noguchi's demonstration of the spirochaete in brain tissues, using the silver impregnation technic and large batches of two hundred slides each. Eckstein tells a dramatic tale of the finding of the organism late at night in the two hundredth slide, the last for that brain. Whether this was actually true or not, I do not know; Eckstein was painting a picture; but I do know that it was true in essence. That was the way Noguchi worked. After the first find, he went back over the slides in which he had failed to find spirochaetes, and there they were in several. Infinite patience and the old story of eyes and no eyes!

In 1912, he wrote his beloved teacher, Madsen, " I am now working on poliomyelitis and rabies although my work on spirochaetes is going on at the same time." He had also begun again his almost perennial attack on the etiology of trachoma. Such a fever of work and such a rash of papers! He reported the cultivation of trachoma [20] bodies from eye lesions; and of Negri bodies from rabies tissues.[21] These papers are

[17] Evelyn Tilden, personal communication.

[18] Hideyo Noguchi: The Spirochaetes. Jordan and Falk, *Newer Knowledge of Bacteriology*. Univ. of Chicago Press, 1928: Chapter 36, pp. 432-496.

[19] Hideyo Noguchi: Additional studies on the presence of Spirochaeta pallida in general paralysis and tabes dorsalis. *Journal of Cutaneous Diseases*, 1913, *31*: 543-549.

[20] Hideyo Noguchi with Martin Cohen: Experiments on the cultivation of so-called trachoma bodies. *J. Exper. Med.*, 1913, *18*: 572-578.

[21] Hideyo Noguchi: Contribution to the cultivation of the parasite of rabies. *J. Exper. Med.*, 1913, *18*: 314-316.

disappointingly brief, that on rabies only three pages, without details, and the one on the cultivation of so-called trachoma bodies also incomplete. In neither instance is there adequate demonstration of a specific relation between the cultivated body and the disease in question. Widely heralded were his more complete reports with Flexner on the cultivation of so called "globoid bodies"[22] from the central nervous system of monkeys paralyzed from acute poliomyelitis. By the injection of material from a few of these cultures they produced the typical disease in several injected monkeys.[23]

For years Noguchi had wanted very much to be married but his formal attempts had been unsuccessful. Finally, April 10, 1912, Noguchi married Miss Mary Dardis, but he kept the fact secret from the Institute and his American friends for a long time. The marriage was a happy, successful one, but childless. "Maizie" aided greatly in keeping him happy through his stormy life.

Triumphal Journey in Europe (1913)

And now the glorious climax of these productive years! He was invited to address the scientists and physicians of Vienna. In that beautiful city, home of so many great physicians, Noguchi was the house guest of Friedrich von Müller. His address (read by another because of Noguchi's poor pronunciation) was greeted with enthusiasm. From Vienna he went to Frankfurt at the request of Paul Ehrlich, thence to Berlin, to Copenhagen, to Stockholm and to London. Dinners, speeches, royal medals and a royal welcome everywhere! Our little mop-haired Japanese scientist, stemming from the poorest illiterate family and suffering always the shame and handicap of his crippled hand, drank deeply of this praise and was uplifted.

Back in New York again, the anticlimax was a bit painful; he was no longer the conquering hero; the poliomyelitis findings were being questioned; the investigations had become a group problem, something Noguchi disliked extremely. He wanted whatever glory the work merited without any distribution. He wrote to a friend, "I have just renewed my rabies work in order to complete it—chiefly the question of the life history of the organism and immunity. . . . Besides I am now asked to

[22] Hideyo Noguchi with Simon Flexner: Experiments on the cultivation of the micro-organisms causing epidemic poliomyelitis. *J. Exper. Med.*, 1913, *18*: 461-485.

[23] Hideyo Noguchi with Simon Flexner: Experiments on the cultivation of the virus of poliomyelitis. *J. A. M. A.*, 1913, *60*: 362.

take up the study of hog cholera. . . . This work will be entirely my own . . . no matter whose name may be inserted in the publication." [24] He is clearly resenting Flexner's name on the poliomyelitis papers. Neither his work on hog cholera nor that on trachoma was going well. An incidental physical examination showed some cardiac hypertrophy and a valvular lesion, so he was cautioned against his usual mode of life, irregular intense activity. He was deeply depressed over the failure of competent investigators, several of them at the Institute, to confirm the poliomyelitis findings. We should recall that Flexner and Noguchi reported the finding of globoid bodies in cultures from tissues and from filtrates of the human and of the experimental disease. With such cultures, the disease was successfully but irregularly produced in monkeys at least twice in series and, in one instance, with the material from the eighteenth transplant generation and, in another, with culture virus that had, in turn, been obtained from an animal successfully infected with the cultivated virus. Other major contributions in this field were those of Amoss,[25] Smillie,[26] and Long, Olitsky and Rhoads.[27] Sparse and irregular were the positive results, and no immunologic relationship between the "bodies" and the disease was established. Especially significant are the findings from the thorough study of the last-named authors. Although they found "bodies" in 11% of their three hundred fifteen cultures and observed that "effects characteristic of experimental poliomyelitis could be induced" with some of these, even to the tenth transplant generation, yet they state that "a study of subplants from these minute, morphological particles did not convince us that we had in hand actual cultures of the globoid bodies, or indeed of any living microorganism." The probable explanation of the "globoid bodies" has been supplied by LoGrippo,[28] who found similar micrococcoid bodies either in the presence, or in the absence, of virus; these bodies passed into suspension from the tissue lipoids, migrated in an electric field like lipoids and took the fat stains,

[24] Eckstein, *op. cit.*, p. 217.

[25] Simon Flexner, Hideyo Noguchi, Harold L. Amoss: Concerning survival and virulence of the microorganism cultivated from poliomyelitis tissues. *J. Exper. Med.*, 1915, *21*: 91-102.

[26] W. G. Smillie: Cultivation experiments on the globoid bodies of poliomyelitis. *J. Exper. Med.*, 1918, *27*: 319-340.

[27] P. H. Long, P. K. Olitsky and C. P. Rhoads: Survival and multiplication of the virus of poliomyelitis in vitro. *J. Exper. Med.*, 1930, *52*: 361-377.

[28] G. A. LoGrippo: Concerning the nature of the globoid bodies. *J. Bact.*, 1936, *31*: 245-253.

Sudan III and osmic acid. He suggests that in cultures by Noguchi's methods the virus may occasionally be adsorbed on such particles.

Although the " globoid bodies " appear to have made their exit, it seems to me highly probable that both Flexner and Noguchi, and Long, Olitsky and Rhoads had some multiplication of the virus in their media which, it must be remembered, contained living cells, not only from the kidney tissue, but also from the ascitic fluid. It is difficult to think of mere survival in the exceedingly high dilutions which the latter authors report, 1.3×10^{-18}, even though one is at the same time aware of the great irregularity of the production of the disease with a highly dilute virus. There is no doubt that the experimental disease produced from the cultured material was poliomyelitis. The methods were not too remote from those that have since then proved highly successful.

A second triumphal trip, this one to Japan (1915) to receive the Imperial Prize of the Japanese Academy, somewhat repaired Noguchi's damaged *amour propre*. He was happy to share his honors with Shika, his devoted mother. He had been given recognition in the United States by being made " full member " of the Institute in July, 1914, with additional assistants and a larger salary, although that could never be enough for Noguchi.

On his return to this country, he tackled Rocky Mountain spotted fever because of its similarity to the Tsutsugamushi disease of Japan, and also infectious jaundice because it was caused by a spirochaete, as shown by Inado and Ido. His conversations with Ehrlich in Europe led him into a flier with arsenicals as possible therapeutic agents against some of his spirochaetes; he hoped the lightning would strike.

Noguchi became seriously ill. He remembered getting a mouthful of *L. icterohaemorrhagiae* while pipetting a culture of that organism, so he thought he might have leptospiral jaundice. But at Mt. Sinai hospital, the diagnosis is typhoid fever, a severe case which goes on to perforation. He made a slow recovery and spent the summer convalescing in the Catskills with Maizie. There he followed the suggestion of a painter friend, Hori, and took up painting himself with excellent success. Hori says, " he would be good at anything."

The Beginning of the Tragedy

In June of 1918, Noguchi went to Ecuador as chief investigator on a commission of the International Health Board to study an outbreak of yellow fever. The story is long and confusing, with chapters continuing

in Mexico, Havana, Peru and Brazil, ending ten years later in Africa where yellow fever had its origin. I shall leave this story to the end because in the intervening years Noguchi plunged into many problems, some quite successfully, and the final chapter in Accra would admirably have suited Aeschylus.

While he was at Lima pursuing his yellow fever work, Noguchi became fascinated by some obscure local diseases called Oroya fever and verruga peruviana, found some cases, and of course, made smears, cultures, and animal inoculations. These materials went into the "future work" pile. He could not push everything along as he used to. The following summer (1921) he enjoyed a characteristic vacation in the Catskills with Maizie, with occasional trips of whirlwind activity in New York. Along with his fishing and painting, he continued writing his monograph on spirochaetes, with some microscopic studies to keep him happy, examining every living animal in the neighborhood for possible new spirochaetes. A revision of his *Serum Diagnosis of Syphilis* and a monograph on yellow fever were also under consideration.

The following winter, he again attacked Rocky Mountain spotted fever, working especially on attempted passive and active immunity to the disease. He did succeed in producing an antiserum [29] of low potency. That year also he began experiments on herpes, but he so disliked the way the work was parcelled out as a group study that he gave it up. He wanted all or none. His yellow fever work was being questioned critically; by the next summer he was again moody and depressed. He played chess with Hori, his friend, the painter, hour after hour, and then they talked and talked far into the night.

While waiting impatiently in Brazil for cases of yellow fever, Noguchi did some experiments with material from measles and from malaria; also trachoma, and hoof and mouth disease. Throughout his life he attacked a variety of different problems at the same time. No problem was too large; by attacking many, he had hopes that one might come to successful fruition even though other excellent investigators had failed with similar methods. He rarely read extensively before beginning his experimentation; he did not wish to know that a given method had failed in the hands of competent men. This proved a handicap to him. He commonly spoke of a "special method," although inquiry showed little if any difference between his procedures and those employed by others.

[29] Hideyo Noguchi: Immunity studies of Rocky Mountain spotted fever. *J. Exper. Med.*, 1923, *38*: 605-626.

A paper on some flagellates from skin ulcers in lumbermen in Brazil drew favorable comment from students of tropical medicine, so off he started on that line again. And in the summer of 1925, he again worked feverishly on Rock Mountain spotted fever with a fresh supply of infected ticks. In a letter to Madsen about this time, he apologized for his neglect of his "old teacher," saying, "I can't get younger but my memories are still young."

Carrion's Disease

He received a tube of blood drawn from a patient dying in Lima, the blood on ice all the way, the diagnosis Oroya fever. He immediately shifted his major efforts and proceeded with renewed vigor to carry out animal inoculations with this blood and with fluid from warts of the verruga. A Peruvian medical student, named Carrion, back in 1885 had reported a self inoculation with fluid squeezed from a verruga wart; a few days later he had died with an acute fever. Most Peruvian physicians therefore thought of the two diseases as different stages of the same infection, called by them Carrion's disease. A group from the Harvard School of Tropical Medicine made an expedition to study this enigma in 1913,[80] and after many experiments had come to the conclusion that actually there were two diseases.

Noguchi succeeded in cultivating from the tube of blood of the Lima case an organism previously described by Barton, *Bartonella bacilliformis*.[81] With this organism, simply by using different volumes and different routes of injection, he was able to produce in monkeys either the skin warts or the fatal anemia. On injection into the veins, an irregular remittent fever resulted; into the skin, a bloody wart, with recovery of the *Bartonella* from each type of the disease. With macerated warts from human cases, he also recovered the organisms and produced the two types of the disease by employing the two different routes of injection. Material from other human cases of both types yielded the organism on cultivation, but different strains of organisms varied mar-

[80] R. P. Strong, E. E. Tyzzer, C. T. Brues, A. W. Sellards, and J. C. Gastiaburu: Verruga peruviana, Oroya fever and uta. *J. A. M. A.*, 1913, *61*: 1713-1716. Also Richard P. Strong: Etiology of Oroya fever and verruga peruviana. *The Harvey Lectures*, 1913-1914, Series 9, pp. 204-223.

[81] Hideyo Noguchi and T. S. Battistini: Etiology of Oroya fever. Cultivation of Bartonella bacilliformis. *J. Exper. Med.*, 1926, *43*: 851-864; also, Noguchi: Etiology of Oroya fever (III and IV). *J. Exper. Med.*, 1926, *44*: 697-713, 715-728; also, Noguchi: The etiology of verruga peruana. *J. Exper. Med.*, 1927, *45*: 175-189.

kedly in virulence, thus giving adequate explanation for the failure of others. Noguchi greatly enjoyed giving his confirmation to Carrion, a lone investigator, rather than to the Harvard Commission.

Trachoma Once More

Noguchi had worked off and on with trachoma for twenty odd years, and he did so want to solve that riddle. Like everyone else, he had had great trouble with contaminating organisms. At this point, he learned of the large number of cases, in all stages, among the Indians of Arizona and was eager to try once more. Again, a cloud-burst of work first in the southwest and later in New York. Characteristically, he isolated and inoculated into many animals each of the cultivated organisms, all but one of them well-known bacteria, secondary invaders that should have been discarded without waste of time. But this one organism was new, morphologically resembling the diphtheria group, but gram-negative and motile.

Inoculations of fresh material from five well developed typical cases failed, but with second generation cultures of the new organism, isolated from four of these cases, slowly progressive granular conjunctivitis with scarring was produced both in monkeys and in chimpanzees. The disease was transmissible in series and when, as commonly, the conjunctiva of only one eye was injected, the disease usually spread to the other eye. *Bacterium granulosis*,[82] as Noguchi now called the organism, was recovered from the lesions with difficulty, but this was successfully accomplished from three rhesus monkeys and one chimpanzee, forty-nine, and one hundred ninety-three days, respectively, after the inoculation. Quoting from Noguchi's monograph, " The histological changes of the experimentally induced conjunctival lesions correspond closely with those of human trachoma and include characteristic follicle and scar formation." Twelve recognized ophthalmologists agreed that the condition in the experimental animals " resembles human trachoma," but the typical vascularized membrane or pannus that is considered pathognomonic for trachoma, was lacking. The antigen-antibody reactions with the organism and serum from human cases were variable.

In a beautifully complete and critical monograph on the " *Etiology of Trachoma* " (1938), Julianelle, chairman of the Trachoma Commission

[82] Hideyo Noguchi: The etiology of trachoma. *J. Exper. Med.*, 1928, *48*: Supplement No. 2, pp. 1-53.

of Washington University, St. Louis, states [83] that initially Noguchi's work, because of his scientific reputation, was widely accepted and stimulated renewed interest in the disease. As investigators worked on, however, they were unable to confirm Noguchi's findings, so that the earlier virus studies of Bertarelli and Cecchetto [84] (1908) and the more completely controlled work of Nicolle and associates [85] (1912-13) came to the front once more. Then in 1935, Thygeson and Procter [86] provided the keystone to the arch by transmitting trachoma to baboons, in five of five instances, by using filtrates from typical cases.

From the enormous and confusing literature on trachoma, agreement has now been rather generally reached that the infectious agent of trachoma is a virus of low infectivity and occasional filterability. A reading of even a portion of this literature including the monographs by Noguchi and by Julianelle gives one renewed respect for the persistent effort of Noguchi and of many less well known investigators striving for an approximation of the truth, in this disease, so important in other parts of our "one world." Noguchi was properly cautious in his published statements, but to his old teacher he wrote freely. "Please accept my apologies for the long silence. Spring worms are gradually breaking through the winter's hibernation. Even in this place, man's spirit is floating between cherry and azalia. The fact that your illness is finally conquered is great rejoicing for small son at great distance. Please at this advanced age be very careful. My health still glowingly good. Day and night I strive. I have just completed trachoma. . . . I am planning to give my first report May 16th. From a sample gotten from western America, I have isolated an organism and injected into chimpanzee and have produced perfect trachoma. . . . Recently I have begun again with Rocky Mountain fever study and I think I will succeed. . . . If I succeed, it will be a noteworthy discovery. Without doubt prevention and treatment will follow. I am quietly waiting. If this proves true, typhus and tsutsugamushi disease will have the road to their causation opened." Poor Noguchi! Always the great man!

[83] L. A. Julianelle: *Etiology of Trachoma.* New York: Commonwealth Fund, 1938, p. 85.

[84] E. Bertarelli and E. Cecchetto: Beitrag zur Aetiologie des Trachoms. *Centralbl. f. Bakt: Originale*, 1908, *47, 1*: 432-440.

[85] Nicolle et alia: Le magot, animal réactif du trachôme. Filtrabilité du virus. *Compt. rend. Acad. d. sc.*, 1912, *155*: 241-243.

[86] Thygeson and Procter: The filterability of trachoma virus. *Arch. Ophth.*, 1935, *13*: 1018-1021.

Yellow Fever

And now to turn back to 1918 when Noguchi, as chief investigator, went to Ecuador to study a new outbreak of yellow fever. It is a painful chronicle in many of its phases but full of human struggle and the tragic drama that has followed Yellow Jack throughout its history. May I confess at once that for much of the decade, I, along with better men such as Castellani and Henry Nichols, accepted Noguchi's work as definitive.

I shall give the ten year history in condensed form. Noguchi knew that leptospirosis existed in the areas where he worked in South America; he knew that even experienced persons had difficulty at times in arriving at a correct diagnosis; he had himself worked with *Leptospira icterohaemorrhagiæ*, in fact he was a specialist in the spirochaete field. Why then did he not lean over backwards to establish that the *Leptospira icteroides* was a different species from *Leptospira icterohaemorrhagiae*? Why did he place dependence on guinea pigs, animals known to be susceptible to the leptospira of Weil's disease and known also to be universally unsuccessful as an experimental animal in yellow fever? I believe that much of the trouble was that Noguchi thought that "Hideyo," the great-man-of-the-world, must succeed. "*Let him that thinketh he standeth take heed lest he fall.*" Was there too much pressure from others? Quite sincerely they hoped that the lightning would strike their expert microbiologist. It did strike, but tragically, rather than as hoped.

Noguchi found spirochaetes similar to *L. icterohaemorrhagiae* in a small percentage of cases diagnosed as yellow fever, six of twenty-seven in the Guayaquil series. He found similar organisms also in some cases in Mexico, in Peru, and in Brazil. He grew these leptospira, injected them into many species of animals, with positive results in dogs, marmosets, and, more acutely, in guinea pigs. Jaundice, ecchymoses, and fatty degeneration of the liver were the chief findings at autopsy, but mid-zonal necroses of the liver, characteristic but not invariable in yellow fever, were not observed in the Ecuador series. Indeed this lesion was not described in the few fatal human cases from which material was obtained in this series. Later in the Brazil series (1924), typical mid-zonal necrosis was well pictured in the successfully infected native monkeys, *Cebus macrocephalus*. Yellow fever had not previously been produced in the species in which Noguchi reported success, and the guinea pig on

which he placed so much dependence has been the animal of choice for the production of experimental leptospirosis (Weil's disease). He produced antisera and vaccines, but in their use the controls were inadequate, and Noguchi was aware of this. His sixth paper [87] was the most crucial. Using the Pfeiffer phenomenon technic in guinea pigs, he reported that blood serum from fifteen of eighteen persons recovering from yellow fever gave a positive reaction. If the dose of the organisms was too large, many of the animals succumbed even though the Pfeiffer reaction had been positive. Control animals, injected with sera from ten non-immune soldiers and two cases of malaria plus the spirochaetes, gave negative Pfeiffer reactions, and all these animals died with typical signs of the disease. That looked good. In this paper also, he showed that "*Leptospira icteroides* would pass through the pores of Berkefeld filters (V and N), and he suggested the possibility of a granular phase of life under certain conditions." In the ninth [88] paper Noguchi described one successful transfer of the leptospira disease from a case of human yellow fever by the bites of stegomyia mosquitoes (*Aedes aegypti*) and two successful transmissions from infected to normal guinea pigs by the same means. Many failures were also reported.

Noguchi and others were convinced, but gradually in Brazil, and in Cuba, papers failing to confirm his findings began to appear. At a symposium in Jamaica (1924), Agramonte,[89] of the successful United States yellow fever commission of 1900, stated that the serological differences between the spirochaetes of yellow fever and infectious jaundice are "not greater than those we find between organisms that form part of a single group, showing them simply to represent different strains." Increasingly, the tide of observations and opinion swung in that direction. Sellards [40] (1926) reported that serum of patients convalescent from typical yellow fever gave negative Pfeiffer reaction to *Leptospira icteroides*, and that this organism and *L. icterohaemorrhagiae* were immunologically identical. The air was filled with gloom.

[87] Hideyo Noguchi: Cultivation, morphology, virulence and biological properties of Leptospira icteroides. *J. Exper. Med.*, 1919, *30*: 13-29.

[88] Hideyo Noguchi: Mosquitoes in relation to yellow fever. *J. Exper. Med.*, 1919, *30*: 401-410.

[89] Aristides Agramonte: Some observations on yellow fever prophylaxis. In *Proceedings of the International Conference on Health Problems in Tropical America*, 1924. Boston: United Fruit Co., 1924.

[40] Andrew Sellards and Max Theiler: The relation of L. icterohemorrhagiae and L. icteroides as determined by the Pfeiffer phenomenon in guinea pigs. *Am. J. Trop. Med.*, 1926, *6*: 383-402.

Africa

In spite of illness in body and spirit, Noguchi realized that it was imperative for him to make a trip to Africa to see for himself. At Accra, everyone cooperated. Again many monkeys, up to 500, hundreds of tubes, thirty helpers at one time, plenty of yellow fever, but no leptospiral jaundice, Weil's disease, and no spirochaetes. Young worked strenuously in cooperating with Noguchi, resolved that no error should occur. Still no spirochaetes. Noguchi became seriously ill, the diagnosis, yellow fever. He said, " It is the end and I want it to be." He died on the twenty-first of May 1928, a disillusioned man, old at fifty-two.

Stokes had died of yellow fever.
Young died of yellow fever.
Paul Lewis died of yellow fever.
Because of his failure,
Noguchi was glad to die of yellow fever.

In justice to Noguchi I must quote a statement made by Theobald Smith at a memorial meeting held in Noguchi's honor at the New York Academy of Medicine,[41] December 20, 1928. He states, " I have read again the papers on his yellow fever studies in South America. I do not see how anyone could have drawn inferences other than he did." In assessing this statement, may I say that having read great numbers of obituaries, I have come to distrust them. We are brought up on the thesis " *De mortuis nil nisi bonum.*" In practice, that principle has come to mean that we exaggerate the " bonum " beyond recognizable fact. Would not a more accurate picture be desirable, more possible to emulate! Smith's remarks, as we would expect, are broadly philosophical, but he could not escape the pressure. He speaks of Noguchi's production of " characteristic lesions of trachoma " in certain monkeys. This was several decades after Nicolle's papers, although several years before those of Thygeson. He also praises Noguchi's " skilful technic " and we know his technic was rather messy, and in closing, Smith prophesies that " he will stand out more and more clearly as one of the greatest, if not the greatest, figure in microbiology, since Pasteur and Koch." I think even our great Theobald Smith has joined Jove and in this case has " nodded."

[41] Theobald Smith: Hideyo Noguchi. *Bull. New York Acad. Med.*, 1929, *5*: 877-884.

Conclusions

How then may we summarize and give appropriate honor where honor is due? Noguchi's outstanding successes were, in my judgment, the cultivation and description of many spirochaetes, the demonstration of *Treponema pallidum* in paretic and tabetic brains, and the clearing up of the enigma of Oroya fever and verruga peruviana. Valuable were his studies on snake venoms, complement and complement fixation, especially in the diagnosis of syphilis. His conclusions in a number of his major investigations, especially those in poliomyelitis, in trachoma, a disease on which he worked periodically for years and intensively during 1926, and his thirty-four papers on yellow fever, the cherished work of his last years, have been proven mistaken. He did so eagerly covet those jewels for his crown! I have never known anyone who grappled with scientific problems so intensely, nor one who so burned to be a "great man." With so many problems under study at the same time, the remarkable thing is that he should have made valuable contributions to so many.

Commonly in Noguchi's letters written in apology to his friends for neglect, or in begging for aid in his difficulties, he would subscribe himself "with bent neck." I am sure that, with painful regret, he would like us to think of him in that manner.

HARVEY IN LONDON *

EDGAR F. MAUER

Harvey's biographical remains are limited and have often been recited. Rather than repeat them it would seem more profitable to inquire into the social circumstances of his time and into the medical and scientific atmosphere in which he worked, so that we may more properly assess the magnitude of his contributions. In going back to the London of Harvey's time and even briefly studying his surroundings, we may each draw certain conclusions about him, his character as a man, and his greatness as a scientist.

In 1602 Harvey graduated, full of honor, from the medical school at Padua. " ' Fair Padua, nursery of arts ' . . . the birthplace of Livy . . . the home of Petrarch . . . the workshop of Giotto and Donatello . . . the university of Dandolo, Vesalius, Fracastorius, Fabricius and Galileo; the medical school of Linacre, Wooton, Caius and Harvey." (1)

Harvey returned to England and settled in London, as did many other young men from small towns like Harvey's native Folkestone. His brothers were already settled there, establishing themselves as successful Turkey merchants, traders with the Levant in spices and silks.

In 1603, a year after Harvey's return from Padua, Queen Elizabeth I died. During her long reign, England had become a great power, her population had increased, and there was a fair measure of prosperity throughout the kingdom. London with its environs had grown to a city of 250,000 of a total English population of four and a half million. London's increase was disorderly and unplanned—resented by the citizens, the Lord Mayor, and the Council whose ordinances and regulations failed to reverse or even stay her growth, that inexorable companion of success. The commerce of London was attractive to large numbers of masterless men, dislocated by the religious upheavals of Henry VIII and the agricultural and economic readjustments of later decades. Competing with these for jobs were discharged sailors and soldiers, released with arms in lieu of pay. They in turn found among the apprentices encouragements and assistance in all sorts of mischief. The citizens of the city—merchants, business men, clerks and artisans—provided the private patients and had attracted physicians and surgeons in numbers sufficient to form here

* From the Library, Los Angeles County Medical Association. Presented 6 June 1957 to the Society for the History of Medical Science, Los Angeles.

the headquarters of their respective guilds, the Royal College of Physicians and the Company of Barber Surgeons. Altruistically chartered to improve their kind and to deal severely with quacks and charlatans, these guilds were to find the realization of their aims long delayed. But as we shall see, the Royal College was of definite assistance to Harvey in the pursuit of his researches. To London also came refugees from the Low Countries, now under the repressive rule of Spain. Here too were the opulent quarters of new nobles; elegant, often cultured, frequently conspiratorial, ending their days in the Tower when unlucky, careless, or betrayed.

It would seem, then, that Harvey had ample reasons for coming to London—his family was here and there were bright opportunities in the practice of medicine. Perhaps he already had his mind's eye set on continuing his researches, inspired by his teacher and friend, Fabricius. If this were his desire, then London was better for his purpose than Oxford or Cambridge, for in those days, their medical schools were not distinguished by faculty, facilities, or accomplishment.

Dr. Lancelot Browne and his daughter Elizabeth may have been further inducement. He may perhaps have known the Brownes prior to his journey across the Alps to Italy in 1598. Certainly on his return, it is obvious that among his friends were people of high station and influence. Dr. Browne was Physician in Ordinary to Queen Elizabeth. Two years after his graduation, Harvey and Elizabeth Browne were married at St. Sepulchre's and set up house in the parish of St. Martin's. Browne then proposed Harvey as physician to the Tower, but despite his influence at court, the application was denied. It was perhaps just as well, for that ancient landmark might well have cast a depressing shadow over the amiable and playful disposition of the young Harvey. Harvey continued then in private practice and succeeded, if one may judge by his patients and his income, which over the years was wisely invested by his commercial brothers.

This was the time of the rich renaissance of English letters, and to London for company, praise and livelihood came poets and essayists, novelists and historians. The grand culmination of this literary revival was the theatre. Dekker, Jonson and Shakespeare were writing and producing the plays we find so admirable. The theatres, despised by the Church and civil authorities, shut down by edict and the plague, were nevertheless the only cultural outlet of popular appeal available to respectable Londoners. It is likely that the Harveys sought relaxation in them. Shakespeare's genius was appreciated by his contemporaries, and as a leading public figure he was quickly made a part of the Londoner's

folklore. A diary records that in the year of Harvey's return, the noted Richard Burbage played the title role in Richard the Third. A citizen's wife was so impressed " that before she went from the theatre she asked Burbage to come to her that night by the name of Richard the Third. Shakespeare, overhearing their conclusion, preceded his star and was entertained. When the message came that Richard the Third was at the door, Shakespeare caused return to be made that William the Conqueror was before Richard the Third." (2)

In this London, a city of tumultuous contrast, Harvey assumed his place as a stable and solid citizen. He applied himself industriously to practice, research, and to preparations for Fellowship in the Royal College of Physicians. First he incorporated his degree with his undergraduate university, Cambridge, in order to satisfy the initial requirement. In those days only graduates of Cambridge and Oxford could achieve election to the governing body of this powerful organization, and early in his career Harvey chose not to remain ineffectual in the onlooker's role of Licentiate. In 1604, two years after graduation, he completed the examinations and, six weeks before his marriage, was admitted a Candidate of the College in ceremonies held in the stone house in Knightrider Street where Linacre had once lived. Three years later, having weathered the prescribed period of probation, he was elected Fellow. Although Fellowship in Harvey's time was restricted to a small group of men, we must not be misled by his desire for that office. Every action following his election revealed his altruism. His ambition was not fired by selfish purposes. On the contrary, he used his position of influence, as we shall see, as a means of improving the intellectual gear of his brother practitioners and the medical care of his countrymen.

In 1609 Harvey applied to the Governor of St. Bartholomew's for the much sought after post of Physician by reversion, this being the method of succession on death or resignation of the incumbent. The quick acceptance of his application is a tribute not only to his ability, but also to the wisdom of the Board of Governors. Harvey began his visits to the patients promptly, first as substitute during the absence of the attending physician, such being the wise method of insuring care of the sick. Six months later, his predecessor Dr. Wilkinson died, and on the morning of October 14, 1609, Harvey was sworn to duty with this charge:

Phisicon: You are here elected and admitted to be the Phisicon for the Poore of this hospitall, to p'forme the chardge followinge, That is to say, one day in the weeke at the leaste thorough the yeare, or oftner as neede shall requyer you shall come to this Hospitall, and cause the Hospitler, Matron, or Porter, to call before

you in the hall of this hospitall such and soe many of the poore harboured in this hospitall, as shall neede the counsell and advise of the phisicon.

He was to care for " the poore then p'sente, or any other of the poore at any tyme in the weeke wch shalbe sent home unto you. . . ." He was further adjured to keep records and

. . . not for favour, lucre or gaine, appoynte or write any thing for the poore, but such good and wholsome things as you shall thinke wth your best advise will doe the poore good, without any affeccon or respecte to be had to the apothecary.

When Harvey took up his duties here, St. Bartholomew's had an unbroken record of service of almost 500 years. It had begun with a vision of the Monk Rahere who was doing penance in Rome. To him one night St. Bartholomew appeared and gave instructions for the building of a priory and hospital in Smithfield, near the walls of the City of London. Rahere returned to England and in 1123 Henry I deeded the property to him. There St. Bartholomew's stands today, well into its ninth century of service to the sick.

One by one over the years, the buildings were erected. There were chapels, a convent, dormitories for the Master and brethren, a hall for the patients' care—and cemeteries for the nuns, the monks and poor, and for those wealthier folk who desired burial here as an earnest of their humility. It is likely that for many centuries the haphazardly placed structures were one-storied, of whitewashed wood.

By Harvey's time the various parts of the hospital were enmeshed in a community which had arisen within and about it; residences and shops of many sorts surrounded and penetrated the grounds. Some of the better houses were leased by tenants who had no official connection with the hospital. One of these early notables was Dr. Caius, founder of Gonville and Caius College at Cambridge and longtime lecturer in anatomy to the Company of Barber Surgeons. By his time some of the houses had grown to three stories and he occupied one in which the hall was large enough to accomodate the entire College of Physicians at their first feast in 1556.

Another famous resident, a patient of Harvey's, was Sir Thomas Bodley, diplomat and scholar, who bequeathed his library to Oxford. These and others of the gentry, living in the better houses, surrounded by pleasant walled gardens, did not inhibit their more zestful neighbors, the shopkeepers and peddlers who noisily sold ale and beer and gin, tobacco and food, to the inmates of the hospital and were a trial to the administration. St. Bartholomew's was indeed a London in miniature.

A number of wards opened on public thoroughfares and since some of the inpatients were removed from the streets by the Beadles of Bartholomew's, it was at times difficult to preserve order. Of these Beadles there were eight, clad in blue coats and carrying staves, and it was their duty to patrol the streets and alleys, avoiding bawdy houses, and to seek out vagrant and sturdy beggars with noisome diseases, and others who, injured or ill, were in need of help or medical care. We can be certain that some of these objects of well intended charity resisted such kindly attentions and, as patients, added color to the wards.

One sometime resident of these parts was Dr. Thomas Doyley, physician to the hospital and, for a while, secret agent for the government in the Low Countries. He once held as prisoner in Bartholomew's the Governor of Dunkirk. Dr. Doyley, who must have been a kindly man, was much concerned for his prisoner's comfort and was disturbed by the conditions under which he was forced to house his captive. Doyley complained that " divers of the poor inhabitants hang their bedding and beastly rags before the prisoner's door and some of the sisters empty foul vessels under his chamber windows." Doyley's discontent was not lessened by the local residents who used the premises as an outdoor laundry. But his concern for his prominent captive marks him as an unusually humane jailer, for he, himself, had once been held captive by this very Governor, who imprisoned him in a state of nakedness.

There were other interesting tenants. Charles Rayner brought against William Clutterbuck, the porter, a charge of keeping a disorderly house frequented by lewd and riotous persons, a charge which fortunately for Clutterbuck could not be substantiated. Another interesting tenant was Mr. Francis who, returning one morning some time after two o'clock as was his custom, was detained, he thought, too long at the gate and beat the sleepy porter on the head. Mr. Francis later came to regret his hasty remedy, for he was not only arrested but forced to apologize, pay the then large sum of four guineas to the hospital, and court costs.

In addition to these internal activities there were other sources of amusement available to the patients, for that large portion of Smithfield, sometimes called Smoothfield, lay directly across the street and was the scene of all sorts of private and public activities. Here, from time to time, the Royal Family came to witness the jousts of their courtiers. Here the famous fairs and markets were held where the products of the Continent were shown. For further diversion there were horse races and occasionally an execution. The latter, attended by citizens of all classes, were occasions of gaiety which belied both the crimes and their

expiation. The general acceptance of execution is illustrated in the case of the Bristol serving girl who, having murdered her mistress, was sentenced to death. Her mother and sisters set off to the execution in high spirits, and on the way one of the girls was heard to call gaily to a neighbor, " Ain't you going to see our Sarah hanged? "

A good deal of activity went on here, directly across the street, and the fun loving Londoners, well armed and approving highly of spirits of all sorts, provided ample work for the surgeons so conveniently at hand.

Here in 1609, five years after his marriage, Harvey found himself as physician. After some deliberation he chose not to live on the premises and obtained instead a rental allowance in addition to his salary, a precedent set by Dr. Doyley whose low opinion of the local inhabitants we already know. It is likely that Mrs. Harvey, daughter of the physician to the Queen, objected to moving here, and her attentive and devoted husband paid heed. But the turbulent atmosphere of the hospital and its surroundings evidently did not disturb Harvey—for he remained as physician for more than 30 years, retiring only because of the disturbances of the Civil War and his attendance on the King in the field.

During his long period of tenure at St. Bartholomew's, he considered many ways by which its services could be improved and established a body of rules, some of which are still observed, containing as they do a good deal of common sense. In the " Charge " previously cited, we have had a description of the way Harvey conducted his clinics. It is a misfortune that his colleagues failed to leave records of his clinical practice. Though he was the only physician, there were a number of surgeons and a bonesetter who treated fractures, until these duties were assigned to the surgeon in 1628. One surgeon was charged with the cutting for the stone by incision in the yard or bladder, the incision of wens, and the cure, by cutting, of rupture or the falling down of the guts into the codds. This division of labor left for the general surgeon the treatment of superficial ulcers, the frequent wounds inflicted on one another by the armed citizens, and the frequent amputations necessitated by infection and injury.

An interesting " colleague " of Harvey's was the widow, Frances Holcombe, who was employed to cure the " scald head " at the rate of 20 shillings the cure, a fancy fee when compared with Harvey's annual stipend of 25 to 33 pounds! In the third year of her employment she was earning more than any of the professional staff. The excellence of her prospects gained her a husband, and the pair are soon found on the rolls of the hospital as lessees of nearby property. Her income rose to £102, while the cost of running the apothecary shop for the entire hospital

of 100 patients was only £21 more during the same year. (3) Paré called the "scald head" by its modern name, tinea, and wrote that "oft-times being healed, it hath left an alopecia behind it, a great shame to the surgeon. Which is the reason most of them judge it best to leave the cure thereof to Empiricks and Women," (4) and which is probably the reason Harvey took no account of the Widow Holcombe in the rules for the hospital.

John Aubrey, (5) that garrulous contemporary biographer, is often uncritically cited as follows: "though all of his profession would allow him to be an excellent anatomist there were not many who admired his therapeutic way," nor would they "give threepence for one of his prescriptions, as a man could not tell by them what he did aim at." Knowing the medicines then in use, we may disregard these criticisms. In the unlikely event that this attitude prevailed, we may be certain that Harvey would have been totally unconcerned, for a man of his scientific tendencies must have had serious reservations about the traditional polypharmacy then in vogue. An example of his practice is shown in the case of Prince Maurice whom he saw in consultation. Maurice was suffering from a serious fever then raging in the army, "and concluding the disease to be venomous it was resolved to give very little physic, only a regular diet." The prince recovered.

The physicians of Harvey's day had for their use the first pharmacopoeia, which was published in 1618. It contained a materia medica of gigantic proportions. There were few mineral products but there were over 600 simples, largely of vegetable origin. It was rich in animalia, in whole or in part, with the addition of excretions and secretions therefrom. Some of the preparations were remarkably complex; for instance the "Great Antidote of Matthiolus against Venoms and the Plague" contained 131 ingredients, some of them in turn compound! The pharmacopoeia also revealed a definite belief in magical properties and listed bone from the heart of the stag, mummy, unicorn horn, and rabbit's foot. It is far more likely that Harvey was dubious of the efficacy of such materials than that he was ignorant of therapeutics, as Aubrey alleged.

On the other hand, it is possible that the apothecaries themselves were responsible for this canard. We may infer that the cost of running the apothecary's shop at St. Bartholomew's was low, because Harvey kept it under control. But if Harvey likewise wrote sensible prescriptions and kept the costs of his private patients' drugs at an economical level, then certainly he must have come into conflict with the unscrupulous, unregulated drug peddlers of the day. Some apothecaries bought prescriptions

from physicians and prescribed them to gullible customers as the best the finest doctors could devise for the ills the apothecaries themselves diagnosed. Such as these split fees, adulterated drugs, and charged exorbitant sums of rich and poor alike. Tobias Smollett a century later still found cause to describe the apothecary in a most unflattering manner. His character, Mr. Lavement, found that "his expense for medicine was not great, for he was the most expert man at a succedaneum of any apothecary in London, so that I have often been amazed to see him without the least hesitation make up a physician's prescription though he had not in his shop one medicine mentioned in it. Oyster shells he could convert into crab's eyes; common oil into oil of sweet almonds; syrup of sugar into balsamic syrup; Thames water into aqua cinnamoni; turpentine into capavi, and a hundred more costly preparations were prepared in an instant from the cheapest and coarsest drugs of the materia medica." (6)

In 1617, the apothecaries were at last granted their own charter and permitted to part from the Company of Grocers, with whom they had been associated for 200 years. Now the Grocers cried in anguish and complained to King James that this was tantamount to starving their children by royal decree. But James held fast, and the Beadles of the new Society of the Art and Mystery of the Apothecaries of the City of London, together with a Censor of the Royal College of Physicians, began the unannounced inspection of the apothecary shops in an effort to stamp out the vices so commonly practiced. Although every description we have of Harvey reveals him as a placid and philosophic fellow, we may well wonder why he permitted himself to be appointed Censor three times. If it was to assist in the reform of the apothecaries, the time he took from his anatomical researches cannot be thought of as wasted. How nice it is to imagine Harvey throwing stocks of Gascoin's powder deficient in Bezoar into the ditch which made its way down the street, or adding supplies of Venice treacle, which had been made on the premises and were of poor body, to the day's refuse! . . . How pleasant it is to think of the apothecary further required by the statutes to add to the ditch each morning and evening his allotment of twenty buckets of water to wash away the rubbish of the day!

It was in such ways that Harvey took part in the reforms of his time, and it was through such streets that he rode on his horse, attended by his footman, on his daily rounds. As he gazed upward, he could see a small slit of sky through the overhanging eaves of the houses, and the sky seemed as polluted as the streets. John Evelyn was soon to complain of

the folly which permitted "Brewers, Dyers, Soap-boilers and Lime-burners to intermix their noisome works amongst dwelling houses in the City and Suburbs," and adding to the "black catalogue" were foundries and the "Fire-engines of the Water Works at *London Bridge* . . . which . . . leave the astonished spectator at a loss to determine whether they do not tend to poison and destroy more of the inhabitants by their Smoke and Stench than they supply with their Water." (7)

The biographers of Harvey have neglected his interest in the public health, but in the "Anatomical Examination of the Body of Thomas Parr," (8) Harvey anticipates Evelyn's "Fumifugium or the Smoake of London." The reader will recall that Parr did more than survive to an exceedingly old age, for in fact he lived to the age of 152 years and 9 months. It is a fairly general misconception that Harvey laid the old man's death to a change in diet incident to his coming to London. Harvey clearly stated, however, that Parr died of a "peripneumony" and that the "chief mischief [was] connected with the change of air, which through the whole course of [Parr's] life had been inhaled of perfect clarity." Now, coming from the country to London at this advanced age, Harvey continues, Parr found himself in "a city whose grand characteristic is an immense concourse of men and animals, and where ditches abound, and filth and offal lie scattered about, to say nothing of the smoke engendered by the general use of sulphureous coal as fuel." Good clinician that he was, Harvey supplied a clinical history with the post-mortem findings and tells us that Parr lived chiefly on dairy products, now held so suspect by modern cardiologists. We are also told that despite, or perhaps because of, this diet, Parr, apparently in celebration of the century mark, transgressed and was forced to do public penance for incontinent behavior. Twenty years later, at the age of 120, his sins being forgotten, Parr took unto himself another wife and was able to support her by working in the fields to the age of 130.

Harvey further records Mrs. Parr's testimony and quotes her as follows: . . . she "did not deny that he had intercourse with her after the manner of other husbands with their wives, nor until about twelve years back had he ceased to embrace her frequently."

Harvey's interest in anatomy and pathology was lifelong and provided, as already noted, another strong reason for his association with the Royal College of Physicians, for in London only the College and the Barber Surgeons were permitted the teaching of anatomy. For this purpose each organization was provided annually with the bodies of four executed criminals. The small number of specimens provided from the large

numbers put to death each year clearly indicates the general distaste for the use of human subjects in the teaching of this important discipline. Capital punishment was common not only for felons but also for those convicted of minor crimes such as petty theft. To the scaffold and the block were delivered many hundreds each year, all selected against their will from the miscreants who at this distance seem a picturesque if destructive lot. Their private Elizabethan argot was almost musical in its alliteration. The names of their specialties are practically endearing, for there were cozeners [1] and cony catchers,[2] bawdy baskets [3] and cut-purses,[4] autem morts [5] and walking morts [6] and whip jacks [7] who with their gybes [8] and jarks [9] generally heaved [10] at fairs, and otherwise preyed upon the citizens of London and the countryside. These malefactors had a minutely stratified and zealously observed social caste system and called their gullible and unwitting providers "Cousins." (9) The few of their number chosen for the anatomies, could they have known, would have been pleased with the ceremonious manner in which their mortal remains were used and the attention they commanded. Attendance by the physicians as well as by the surgeons at their respective official anatomies was compulsory, and evasion was punished by heavy fines. To these affairs the public was invited, but they were called public anatomies because the body of a felon was used, and Pepys left a good account of them, priding himself on his courage in touching one of the subjects. Traditionally these anatomical sessions lasted for three days with a lecture in the morning, an elaborate feast at noon, and another lecture in the afternoon. The best teachers were obtained and provided with a liberal honorarium. Ample assistants were on hand who helped with the dissection and in addition were, by regulation, charged with the responsibility of providing a rug to guard the lecturer's feet from the cold, sufficient sets of sleeves to protect his fine clothing, and instruments in proper order to permit the dissection to proceed expeditiously. Usually each lecture was given by an expert known to excel in the prescribed field, but occasionally one, such as Harvey, was engaged who could do the entire body to his auditors' satisfaction.

The earliest privately endowed lectures in England were the Lumleian,

[1] cheats, [2] confidence men, [3] women peddlers, [4] cut purses.
[5] married women who travelled with any of the preceding.
[6] unmarried women who did the same.
[7] thieves who frequented public places.
[8] any writing, particularly a license for begging, teaching the catechism, etc.
[9] a seal affixed to a license or pass.
[10] stealing goods from booths at fairs or markets.

and an account of the founding of this series in relation to Harvey is important. In 1578 Master Doctor Caldwell, Doctor in Physic, offered to the Company of Barber Surgeons an endowment of £10 annually for the establishment of a surgical lecture. Precisely why the physician Caldwell made this offer to the surgeons is not clear, but at any rate they failed to accept the subsidy, probably because this sum was not sufficient to defray the expenses of the customary banquet. Their failure to accept this offer was short-sighted, for had they agreed, the Lumleian Lectures and Harvey might now be associated with the surgeons. Two years later, Dr. Caldwell and Lord Lumley of Essex established the lectureship at the Royal College of Physicians, to continue in perpetuity, with an annuity of £40 to be derived from certain rents belonging to the donors. The lecturer was to be appointed for life and was to receive a stipend no less than that of the Regius Professors of Law, Divinity or Physic at Cambridge or Oxford. This stipend had been set by Henry VIII at £40 per annum, and although the annuity did not meet all of the costs, the College accepted it. The form and content of the lectures were prescribed. They were to be given each Wednesday and Friday morning throughout the year from 10 to 11 o'clock, and were to cover, in six year cycles, current knowledge and certain of the ancient authorities. At the end of each year the lecturer was to dissect a body over a period of five days, "as well before as after dinner; if the bodies last so long without annoy." The form of the lectures changed from time to time, but the content was adhered to.

The audience too was subject to ceremonial procedure. Admission was by ticket, and all wore their academic regalia, some with caps, others without, in accordance with regulation. At the proper hour they assembled, formed ranks and strode with dignity into the chamber. In August 1615, as the Physicians and their guests proceeded to their chairs, they were faced by a new lecturer, for William Harvey had succeeded to the Lumleian. There of a morning in the new theatre of the College in Amen Corner at the end of Paternoster Row, he would stand, awaiting his auditors, the body before him, his attendants at his side. And here one morning in April, 1616, probably Wednesday, the 17th, he spoke to his audience. The short, dark skinned lecturer, his eyes black and spirited, his hair raven and curling, seemed to his listeners no different from the Harvey of the previous day. Although he made no outward sign, we may well wonder what his thoughts were, when, having carefully described the functions of the various parts of the vascular system, with asides on

the circulation in the human foetus and that of birds, fishes and reptiles, he read these lines from his notes:

> It is plain from the structure of the heart that the blood is passed continuously through the lungs to the aorta as by two clacks of a water bellows to raise the water . . . it is shown by the application of a ligature that the passage of the blood is from the arteries into the veins. . . . Whence it follows that the movement of the blood is constantly in a circle and is brought about by the beat of the heart. (10)

In these simple terms he described the conclusions drawn from his observations and experiments. Here was in fact a revolutionary concept which explained many of the phenomena which his auditors must have witnessed but to which they had paid no heed, for they failed to observe what did not fit into the scheme of things they had been taught to accept. Here was a statement which bore not the slightest resemblance to the ideas which physicians for more than a millenium and a half had learnt, and which they in turn had passed on as the truth. Harvey, having thought that he "had attained to the truth," wrote:

> I have not hesitated to expose my views upon these subjects, not only to my friends, but also in public in my anatomical lectures, after the manner of the academy of old. . . . Some chid me and some calumniated me and laid it to me as a crime . . . to depart from the precepts and opinion of all anatomists: others desired further explanations. . . . (10)

Harvey paid no heed to his detractors—to those who called him "crack-brained." When, finally, *De Motu Cordis* was published, twelve years after he first gave his views to a public audience, it was buttressed with proof obtained by research on animals, by clinical observations, and by every resource his great mind could bring to bear in support of his argument. Now there were no cries of priority, for the proponents of authority were too preoccupied with the discomforts involved in adjusting to a new idea to trouble themselves, as did their followers, with chauvinistic claims for glory.

It has been said that only those under the age of forty accepted Harvey's discovery. But Harvey dealt gently and intelligently with his older brethren, "aged not so much by weight of years as by preconceived opinions," to use the description of Madame Schopenhauer; she went on to say of 18th Century German doctors that "they were all aged." She "could not imagine that any of them had ever been young, or had condescended to the sports of the young. For many years of her life she never either saw or heard of a young physician." (11)

Harvey had, however, his disciples, men of vision with clear minds who

recognized his greatness. In the total body of his published works—the *Prelectiones, De Motu Cordis, De Generatione*, the replies to Riolan, and the few letters which remain, Harvey reveals himself as more than the discoverer of a great truth and a new method of science. In these works he literally shines with modesty, good humor (although Aubrey called him choleric) and humanity. "Motion from the lower Hous sic acidi humores: acidi fawces" (12), i. e., an acid taste rising from the stomach into the mouth reminds him of a motion passing from the lower to the upper house of Parliament. But alas, he too was human; he could not bring himself to look upon Aselli's description of the lacteal vessels with favor, but it may be said in extenuation that it is clear that he sought a simpler mechanism and believed that one system served nature's purpose better than two.

It is fair to say that by 1640, at the age of 62, Harvey's productive life ended, though *De Generatione Animalium* was not published until 1651. The serenity of his life was disturbed by the Civil War, beginning in 1640. His papers were destroyed by the Parliamentary troops. Perhaps because of these disturbances, his *Medical Observations*, which he meant to publish, were tragically lost. In Harvey's literary remains are clear evidences of a wide interest and provocative glimpses of clinical descriptions and interpretations which might have been ours to read had this material survived. But Harvey was disturbed most by the execution of his friend and patron, Charles I, who had honored him with the appointment of Physician in Ordinary in 1631.

Harvey survived his tribulations. He lived to see his discovery accepted in most of the medical schools of the continent. He became an object of veneration to his colleagues in the Royal College, and for them he built and endowed a library which was destroyed in the Great Fire of 1666. When elected President, he refused because he felt himself too old to serve well. He spent the years after the civil war in retirement in quiet contemplation at the various country estates of his brothers. On June 3, 1657, nearing his 80th year, he died. His body was conducted far beyond the city walls by the Fellows, on its way to the family vault at Hempstead in Essex.

Let us return to 1628 and *De Motu Cordis*. Harvey was fifty years old, and by every criterion a success. He had a good practice, financial security, and was respected as a citizen and physician. He held with distinction the several appointments of which we have heard. Such rewards come often to the gifted and vigorous. But to Harvey there came the far greater distinction, that of discoverer, of innovator, and under circumstances

which make his accomplishments memorable. The weight of authority lay on him as heavily as it did upon his fellows, and the break from the past was far more difficult than it was to become through the efforts of men such as he. In his own country science was only then being born. John Gilbert, also a physician, friend and colleague of Harvey's father-in-law, was the first scientist of the island empire. In 1600 he had published *De Magnete*, a work which was based on one of the earliest uses of the experimental method. Harvey may have known him.

And there was Bacon, who was to become Harvey's patient. Bacon's dismay with the sterility of man's mind revealed itself early, for at the age of 16 he fled from Cambridge "disgusted . . . with the dialectic of the schools." (13) He joined the embassy at Paris where he may have known Palissy. Surely he knew of his works, for this 16th century potter, self-educated, sought by the methods of the laboratory to recapture the glazes of Faence and Della Robbia. In that quest Palissy established for himself the experimental method. Palissy was lecturing on his new method of science while his contemporaries were lost in alchemy. Bacon may have heard him. In a little while, Bacon wrote of the smothering influence of authority, of the fetters of tradition. He urged the investigation of natural phenomena, the use of experiment, the development of basic knowledge, and, if further proof is needed of the lasting worth of Bacon's counsel, let us remember that he advised subsidies for the scientists who were to bring into being his new world.

On the Continent, Vesalius had started anatomy in a new direction, using investigation as his tool and scepticism as his stimulus. Although Servetus and Columbus may be said to have understood the pulmonary transit of the blood, the anatomists who followed Vesalius to the time of *De Motu Cordis* were concerned almost entirely with structure. Medicine, in the absence of etiologic knowledge, remained in a static condition. The refining influence of Sydenham was still in the future. Medical treatment was empiric, often relying for its benefits on the magical properties of imaginative remedies. In the surgical arts some gains had been made, but these gains were the results of trial and error methods, not to be diminished thereby, but not yielding any basic principle or general truth. Copernicus, Galileo, and Kepler had created the new astronomy, but a century later, during Harvey's last years, the schools still ignored Copernicus and taught the geocentric notions of the ancients. Copernicus knew well the dangers to be confronted and delayed the publication of his doctrine until the hand of age was well upon him. Thanks to his caution, he died in his bed. Had not Galileo recanted, he would have gone to the

stake, as did Servetus, Bruno, and Dolet. The printing press, that most wonderful instrument of learning, was rigidly controlled by censorship and a stringent code of licensure. In Harvey's own land and in his time witchcraft was a capital crime.

It was into such a city and into such a world that William Harvey through observation and experiment brought his discovery and revealed it for all to see. He had the courage to confront his fellows with the errors of the past, to defend his views with reason, and the good fortune to survive because a measure of intellectual freedom had come to his land.

REFERENCES

1. Newman, Sir George, *A Century of Medicine at Padua.* London, British Periodicals, Ltd., 1922.
2. Harrison, G. B., *The Elizabethan Journals (1591-1603).* London, Routledge, 1938.
3. Paget, James, *Records of Harvey*: In Extracts from the Journals of the Royal Hospital of St. Bartholomew. London, Churchill, 1846. (Reprinted in *St. Bartholomew's Hospital Reports*, 1886, *22*, 1-37).
4. Parey, Ambrose, *The Workes of that Famous Chirurgion Ambrose Parey.* Translated out of Latine and compared with the French by Th. Johnson. London, Cotes and Young, 1634.
5. Aubrey, John, *Brief Lives.* (ed. Oliver Lawson Dick), London, Secker and Warburg, 1950.
6. Smollett, Tobias, *Roderick Random.* New York, The Jenson Society, 1907.
7. Evelyn, John, *Fumifugium: Or, The Inconvenience of the Aer, and Smoake of London Dissipated.* London, 1772. (Old Ashmolean Reprint v. 8), Oxford, Günther, 1930.
8. Harvey, William, Anatomical Examination Of The Body Of Thomas Parr, in *The Works Of William Harvey, M. D.*, Translated by Robert Willis. London, The Sydenham Society, 1847.
9. Judges, A. V. (ed.), *The Elizabethan Underworld.* London, Routledge, 1930.
10. Power, D'Arcy, *William Harvey.* New York, Longmans, Green & Co., 1898.
11. Doran, J., *Habits and Men*, p. 383. New York, Redfield, 1855.
12. Harvey, W., *Prelectiones anatomiae universalis.* Edited with an autotype reproduction of the original by a committee of the Royal College of Physicians of London. London, J. and A. Churchill, 1886.
13. Allbutt, Sir Thomas Clifford. *Palissy, Bacon, and the Revival of Natural Science.* London, Oxford University Press, 1913. (Proceedings of the British Academy 1913-1914)

The author is also indebted to the following works for much of the material not cited in the references:

Chapin, Arnold. *Medicine in England During the Reign of George III.* The Fitzpatrick Lectures delivered at the Royal College of Physicians 1917-1918. London, 1919.

Moore, Norman. *The History of St. Bartholomew's Hospital,* 2 vols. London, Pearson, 1918.

Power, D'Arcy, *A Short History of St. Bartholomew's Hospital, 1123-1923.* London, printed for the hospital, 1923.

Thompson, C. J. S., *The Mystery And Art of The Apothecary.* Philadelphia, Lippincott, 1929.

Wilson, F. F., *The Plague in Shakespeare's London.* Oxford, Clarendon Press, 1927.

THE CHOLERA EPIDEMIC OF 1832 IN NEW YORK CITY

CHARLES E. ROSENBERG

Survivors do not easily forget a cholera epidemic and to a generation of New Yorkers 1832 became the cholera year. For cholera is an unnerving disease, its symptoms revolting, its prognosis discouraging, its etiology an indictment of the society which harbors it; a grim reminder of man's mortality, it could not be ignored, or treated, or prayed away.

Cholera was a new disease to Americans in 1832.[1] Before 1817 there had probably never been an epidemic of cholera outside of the Far East—during the nineteenth century it spread through almost the entire world. Of all epidemic diseases, only influenza in the nineteenth century has had a more extensive argosy. The terrifying, disruptive course of this new disease had somehow to be allayed, to be made understandable. Of all exercises in humility, that of admitting ignorance and helplessness in the face of imminent death is one of the most difficult. Cholera had somehow to be explained. The purpose of this paper is to discuss some of the ways in which one community attempted to fabricate a consistent conceptual apparatus with which to explain the inexplicable, provide security where there was only uncertainty.

Though they had never seen it, cholera was not an unknown quantity to well read New Yorkers, who had followed its seemingly ineluctable progress from India until, in 1831, it reached England. As early as September 6, 1831, the Board of Health resolved that three of the City's most prominent doctors be requested "to collect . . . with all possible speed, all information which can be obtained from Europe or Asia or elsewhere on the subject of the Oriental Cholera."[2] A second step in the prevention of cholera was the tightening of quarantine regulations.

[1] The secondary literature on the epidemic in New York City is not extensive. Accounts of it may be found in J. S. Chambers, *The Conquest of Cholera: America's Greatest Scourge*. (New York, 1938) pp. 48-67; I. N. P. Stokes, *The Iconography of Manhattan Island* . . . (New York, 1926) V; U. S., *The Cholera Epidemic of 1873 in the United States*, 43rd Cong., 2nd Sess., Exec. Doc. #95 (Washington, 1875), pp. 566-569 and *passim*. Contemporary accounts are of course quite numerous. A satisfactory guide to them may be found *ibid.*, pp. 727-736.

[2] David Hosack was one of the three. Minutes of the Board of Health, September 6, 1831. MSS. City Clerk's Papers, Municipal Archives and Records Center. For a detailed account of the epidemic and its preliminaries see Charles E. Rosenberg, "The Cholera Year: The Cholera Epidemic of 1832 in New York City" (Master's thesis, Columbia University, 1957).

In the past, restriction had been applied only during the summer months But cholera, unlike yellow fever, seemed to show no preference for warm climates: it had raged in Moscow in mid-winter, and quarantine regulations were maintained in New York throughout the winter of 1831-1832.

The Board of Health had also formed a committee with powers to "memorialize" the President and General Congress,[3] but the new year brought with it news that cholera had broken out in England, and to many Federal aid offered little hope. Could the clumsy hands of government avert an epidemic influence which in fourteen years had made its inevitable way from the Ganges to the Thames? A *cordon sanitaire*—enforced by heavily armed troops—had failed to halt the spread of the disease westward from Poland and Russia. Quarantine restrictions seemed to be of no avail and, as the summer of 1832 approached, it appeared more than likely that New York, like Moscow, Paris, and London, would be visited by this newest judgment. Only the Atlantic Ocean continued to protect the United States.

New York City, in the winter and spring of 1832, provided an eager audience for scores of articles and treatises on the cholera. For once the disease had reached England, it seemed almost inevitable that the United States should, in its turn, fall victim. A newspaper account of a medical school commencement was entitled "Providing for the Cholera." In March, over two thousand crowded into the Baptist Meeting House on Mulberry Street to hear Sylvester Graham lecture on the cholera. Even Tammany was alarmed: a new law was passed to regulate the cleaning of the city's streets and the Mayor increased the severity of the already severe quarantine.[4] On the fifteenth of July the threat became more real and more imminent. The Albany steamboat which docked that Friday afternoon brought the news that cholera had broken out in Montreal and Quebec. The Atlantic had been forded—America's last great defense had failed, and it hardly seemed possible that New York would be spared.

New York was not a large city, and by Saturday morning, June 16, nearly everyone had heard the news from Canada; news which evoked a

[3] Board of Health "Minutes," MARC, January 4, 1832; U. S. Congress, *Cholera Morbus*, 22nd Cong., 1st Sess., January 20, 1832. House Report #226. The Committee on Foreign Affairs to which this petition was referred, reported negatively, but was careful to affirm the right of the Federal Government to impose quarantines.

[4] New York *Courier and Enquirer*, April 6, 1832; Sylvester Graham, *A Lecture on Epidemic Diseases Generally and Particularly the Spasmodic Cholera* . . . (Boston, 1838); New York *Evening Post*, February 3, 1832; N. Y. C. Board of Assistants, *Report of the Committee on Cleaning Streets*, Doc. # 36, 1832 (New York, 1832).

mood alternately cynical and despairing. Within two weeks the fears of the most despairing seemed to be materializing.[5] On Sunday, July 2, despite a calculated official silence, the existence of the first cases of cholera in the city was an open secret. Mass exodus from the city had already begun. To those able, flight was the immediate and traditional reaction. A hyperbolic and sarcastic observer remarked later that on Sunday "fifty thousand stout-hearted . . . New Yorkers scampered away in steamboats, stages, carts, and wheel barrows." Farm houses and country homes within a thirty mile radius of the city were filled. Yet the exodus continued, despite the almost uniform hostility with which the refugees were greeted when they tried to enter, or even pass through, any other town. Those who scorned this precipitate flight and elected to remain, tried to expose themselves as little as possible. A merchant living on one of the principal residential streets recalled that his, and one other family, were the only ones on the street to remain. For six weeks, he and his Negro servant were the only persons to leave the house and then only in the morning to buy food.[6]

The poor, having no choice, remained. Living in crowded, filthy rooms they were perfect victims for cholera. Basement apartments were from four to six feet below the surface of the ground, and from these warrens came the "greater proportion and worst form of cases." On July 20 over a hundred deaths were reported and the city seemed almost in chaos. Cartloads of coffins rumbled through the streets, and, when filled, returned through the streets to the cemeteries. Dead bodies lay unburied in the streets and coffin makers had to work on the Sabbath to supply the demand. Harsh smoke from burning clothes and bedding filled the air, competing with the acrid fumes of tar, pitch, and other time-tested preventives. Houses stood empty, prey to dust, burglary and vandalism. By August, even churches were beginning to shut their doors—especially those with wealthier congregations.[7]

[5] It is more than likely that cholera was imported into New York independently of the outbreak in Canada. The news of the arrival of infected ships in New York harbor was suppressed, while the timing of the first cases in New York City precludes their having had their origin in the St. Lawrence episode. Alexander Vache, *Letters on Yellow Fever, Cholera and Quarantine* . . . (New York, 1852) 47n.; U. S., *The Cholera Epidemic of 1873 in the United States*, 43rd Cong., 2nd Sess., Exec. Doc. #95 (Washington, 1875) 567, 8; Rosenberg, "Cholera Year," pp. 17-21, contains a discussion of this problem and of the efforts by the City government to avoid any announcement that the epidemic had broken out. Cf. Chambers, *Conquest*, pp. 57-59.

[6] New York *Evening Post*, July 13, 1832; Philip Hone, Diary, microfilm, NYHS, July 1, 1832; N. T. Hubbard, *Autobiography* . . . (New York, 1875).

[7] Board of Health, "Minutes," MARC, July 18, 1832; Henery Antice, *History of St.*

Fortunately, the disorganization of the city never became complete anarchy. Respectable persons of regular habits were convinced that they had little to fear from the disease. Only the filthy, the intemperate, those who had somehow predisposed themselves, were its intended victims. The Special Medical Council announced on July 10, a day on which there had been forty-five deaths, "that the disease in the city is confined to the imprudent, the intemperate, and to those who injure themselves by taking improper medicines."[8]

Medical opinion was unanimous in agreeing that the intemperate, the irregular, the filthy were peculiarly vulnerable. Cholera was an influence in the atmosphere; debilitating, but malignant only to those who had somehow weakened themselves. Nor was it difficult to weaken oneself, to render oneself a fit subject for cholera; the "predisposing" or "exciting" causes were as varied as the occasions of sin. Any irregularity or excess could provoke an attack. In this doctrine of predisposing causes, the needs and attitudes of an awakening science found reconciliation with the primitive, and reassuring, principle of sin as a cause of disease. Cholera was not a scourge of mankind, but of the sinner.[9]

Asiatic cholera was a disease, not only of the sinner, but of poverty, circumstances easily reconciled, for neither poverty nor wealth was an accidental condition. Many of the well-to-do saw in their wealth testament to the virtue and regularity of their habits. On the other hand, the fact of poverty was in itself something damning and immoral. It was no matter of chance that so many cholera cases came from the ranks of the poor, the morally abandoned of society. The very vices—intemperance, irregularity and immorality—which doomed a man to poverty were the same ones which predisposed him towards cholera.

Cholera did, in fact, find a disproportionately large number of its victims among the lower classes, a fact verified for every cholera epidemic in

George's Church in the City of New York 1752-1911 (New York, 1911), pp. 132, 3; New York *Gazette and General Advertiser*, July 18, 1832; N. Y. C. Board of Health, *Reports of Hospital Physicians and other Documents . . .*, ed. by Dudley Atkins (New York, 1832), pp. 14, 15.

[8] Board of Health, "Minutes," MARC, July 10, 1832. The Special Medical Council was a group of seven physicians appointed by the Board of Health to act in an advisory capacity during the epidemic.

[9] A short history of this concept has been written by Wolf von Siebenthal, *Krankheit als Folge der Sünde* (Hannover, 1950). Cf. also Forrest E. Clements, *Primitive Concepts of Disease* (Berkeley, 1932) and Dorothy M. Spencer, "Disease, Religion and Society in the Fiji Islands," *Monographs of the American Ethnological Society* (New York, 1941).

which statistics are available. In Hamburg, in 1892, the case rate among those with an income of a thousand marks was nineteen times greater than the rate among those with an income of fifty thousand marks. In 1831, in Paris, there were so few cholera deaths outside the lowest classes, that the poor regarded the epidemic as a poison plot fomented by the aristocracy and executed by the doctors. In New York, in 1832, almost all of those who died during the epidemic were buried either in Potter's Field or at St. Patrick's. Of one hundred who died of cholera on one July day, ninety-five were buried in Potter's Field.[10]

Even before cholera had reached North America, informed New Yorkers were convinced that only those of irregular habits had anything to fear. It was a matter of common knowledge that out of " fourteen hundred lewd women in one street in Paris, thirteen hundred died of cholera." In some European cities it had been an exception for drunkards to survive a cholera epidemic. Could there be any doubt that the prostitute, the drunkard, the glutton were its foreordained victims? The editor of one New York paper could not credit letters from Canada reporting that cholera was beginning to attack the better sort. Not knowing the writer, he could scarcely credit such unlikely statements.[11] When the disease did come to New York, its progress seemed to confirm the conviction that cholera was a scourge of the immoral. Alexander H. Stephens, President of the Special Medical Council, wrote that the " Disease had been confined to the intemperate and dissolute with but few exceptions." In one house on Laurens Street, thirteen prostitutes had been attacked, and of these all but three had died immediately. Indeed, the history of the epidemic was again and again a source of reassuring and moral *exempla*:[12]

> Notwithstanding the increase of sickness and death, every day's experience gives us increased assurance of the safety of the temperate and prudent, who are in circumstances of comfort; and what families of this character not in such circumstances. The disease is now, more than before rioting in the haunts of infamy and pollution. A prostitute at 62 Mott Street, who was decking herself before the glass at 1 o'clock yesterday, was carried away in a hearse at half past three o'clock. The broken down constitutions of these miserable creatures, perish almost instantly on the attack. . . . But the business part of our population, in general appears to be in perfect health and security.

---

[10] Bernhard J. Stern, *Society and Medical Progress* (Princeton, 1941), p. 134; Ashbel Smith, *The Cholera Spasmodica as Observed in Paris in 1832* . . . (New York, 1832); Board of Health, Report of the Potter's Field Keeper, undated. File Drawer U57, City Clerk's Papers, MARC.

[11] New York *American for the Country*, June 26, 1832.

[12] New York *Commercial Advertiser*, July 12, 1832; New York *Mercury*, July 18, 1832.

The death of any person of consequence was an immediate cause of consternation; a consternation invariably allayed by reports that this usually praiseworthy man had either some secret vice, or else had indulged in some unwonted excess. To die of cholera was to die in suspicious circumstances.

In general, indiscretions in drink and diet were regarded as the most important predisposing causes: a pineapple or a melon was a death warrant, a dozen oysters suicide, overindulgence in alcohol the most dangerous of all. It was argued that few children died of the disease—which was not true—since they were not yet habituated to strong drink. Hospital statistics showing the prevalence of intemperance in cholera victims were widely reprinted. In one institution 198 out of 204 cholera patients were "found" to have been intemperate: an encouraging fact, although the method of obtaining such information from cholera victims, often *in extremis*, remains something of a mystery.[13]

John Pintard, a stern and upright man, wrote on July 13, that the alarm in the city would be indeed great if the disease were ever to attack the "regular householders." He thanked God that it remained "almost exclusively confined to the lower classes of intemperate dissolute and filthy people huddled together like swine in their polluted habitations." A week later, at the very height of the epidemic, Pintard was still calm. He wrote to his daughter that those attacked were "chiefly of the scum of the city," and that the sooner this group were dispatched, the sooner the disease would run its course. One anonymous newspaper article likened cholera to syphilis—scourges created to bring retribution specifically to the transgressor of moral law. Even if New York had to mourn the loss of some estimable and valuable citizens, it would be "mere affectation" not to acknowledge that hundreds had been removed "who were festering wounds in the face of society." [14]

Thus, to men like Pintard, cholera had two divinely ordained functions. One was to demonstrate to man the power of the Lord and the futility of earthly values. The second was—in the words of Gardiner Spring, pastor of the Brick Presbyterian Church—to "promote the cause of righteousness, by sweeping away the obdurate and incorrigible . . . to

[13] New York *Mercury*, August 8, 1832.

[14] John Pintard, *Letters from John Pintard to his Daughter Lisa Noel Pintard Davidson 1816-1833* (New York, 1941), vol. IV, pp. 72, 75. July 13 and July 19, 1832; New York *Mercury*, August 1, 1832.

drain off the filth and scum which contaminate and defile human society." [15]

There is a class of men whom nothing will reform; who live only to degrade themselves and their fellow men, and whose only influence is to pollute and poison the fountains of joy. Towards such persons the God of Mercy is not wanting in forbearance; but there is a limit beyond which his forbearance may not be extended. . . . It is a kind interposition of His Providence towards the cause of religion and good morals, when men who are the pests of human society, and whom nothing can reform are swept away by divine judgments.[16]

But there were many other religiously oriented thinkers who could not share these harsh beliefs, who could not conceive of the God of mercy as a vengeful Old Testament War Lord, interposing himself in temporal affairs and punishing the sinner with death. To these men, the pestilence was an inevitable result of man's failure to observe the laws of nature; laws which had been established by God for man's own benefit. Man has free will and when he fails to observe these immutable laws he brings inescapable punishment upon himself. Cholera was brought about by intemperance and filth and vice, conditions which had never been imposed by God. " If man will close his eyes . . . and stumble on in opposition to the immutable laws of God, he must bear the pains and suffer the penalties of such waywardness." Only miracles could be said to come directly from the hand of the Lord, only those events which could not be explained on the basis of natural laws. " Famine, sword, or pestilence may depopulate a nation, and no link in the thousand stranded chain of causes be displaced or superseded." Although punishment did not come directly from the hand of the Lord, it was nevertheless a consequence of the actions of man, of his individual and collective sins.[16]

This attitude, more common than the stoic Calvinism of a Pintard, pointed clearly towards meliorism and social criticism. The rich as well as the poor were responsible for a society in which God's natural laws were continually broken, even if it was the poor who suffered most directly.

> Yet still will wealth presumptuous cry
> What, though the hand of death be thus outstretched
> It will not reach the lordly and the high

---

[15] Gardiner Spring, D. D., *A Sermon Preached August 3, 1832, a Day Set Apart in the City of New York for Public Fasting, Humiliation, and Prayer* . . . (New York, 1832).

[16] From a sermon by William Whittingham of St. Luke's. Mrs. H. Croswell Tuttle, *History of St. Luke's Church in the City of New York 1820-1920* (New York, 1927), reprints the sermon, pp. 496-511.

> But only strike the lowly and the wretched,
> Tush!—what have we to quail at? Let us fold
> Our arms, and trust to luxury and gold.
>
> O thou reforming cholera! thou'rt sent
> Not as a scourge alone, but as a teacher. . . .[17]

Still, to these men, the epidemic was a judgment, but a judgment condemning an entire society, not merely the individual sinner.

> Atheists may deny, but the intelligence and piety, the real wisdom among us will acknowledge the providence of God; and this acknowledgement will be made by the great majority of our population. They *feel* that God is chastising us.

Pestilence, like war and famine, was a "rod in the hand of God;" a final resort of the Deity, an appeal to man's fears when there seemed no recourse in appealing to his gratitude or hope. "Fear is the basest passion of our nature to which motives can be addressed, but it is often the only avenue to the soul."[18]

Extremists in the Jacksonian camp were quick to indict the society in which they lived; to disassociate God from the calamity; to charge that the Deity was a mere red-herring dragged in to obscure the real causes of the epidemic. George Evans proposed a graduated income tax to provide the funds necessary to make the recurrence of *any* epidemic impossible. For Evans believed that the origin and spread of cholera, and of diseases in general, was due to "poverty, occasioned by unjust remuneration of labor."

For many ordinary men, not professed and spectacular radicals like Evans, the cholera epidemic was a time of realization and disillusionment. They could not help observing the relative immunity of the wealthy, nor help concluding that if all were in such positions of leisure, the cholera would scarcely, if at all, be a problem. A Quaker merchant, writing to a friend in Pennsylvania, expressed clearly the reactions of a sensitive man faced everywhere by selfishness and inhumanity:

> Oh . . . when will it be otherwise? Was man made for this? Or is he not able to raise himself out of this unequal degraded state? If not, better be a stone destitute of sensibility. . . .[19]

---

[17] New York, *The Atlas* (1831-1832), *4*: 169.

[18] New York *Commercial Advertiser*, August 2, 1832; New York *Mercury*, July 25, 1832; Gardiner Spring, *A Sermon*. . . .

[19] New York *Sentinel*, July 17, 1832; New York *Standard*, July 18, 1832; Amos Gilbert to Adam Witmer, July 26, 1832. Misc. MSS. NYHS.

Regardless of the implications of theoretical orientation, none could remain idle while the sinner perished, no matter how grievous his transgressions. In the writings of scores of different men, I have not found one who believed the cholera to be a God-sent scourge, who did not also believe in precisely the same prophylactic measures as those who considered the disease to have its origins in temporally remediable factors. Common humanity, allied with primal fear, demanded that prompt and effective action be taken.

The reassurance garnered from the doctrine of predisposing causes, and from belief in the non-contagiosity of the disease, was vitiated by the conviction that cholera could become indiscriminately virulent if it were to rage uncontrolled in particularly filthy and confined plague spots. The epidemic "influence" generated under such circumstances might well prove fatal even to those of regular habits. It was clear that the "moral fens and morasses" of society were potential sources of danger to every class in the community. As might have been expected, the progress of the disease demonstrated the reality of this danger. At first limited to the poorest and most miserable quarters of the city, the disease soon began to spread outward from these foci of infection. A "Physician" urged in an open letter to the Board of Health that Laurens Street, one of these foci, be evacuated, "before the entire street was depopulated." Even if the inhabitants were poor and idle and drunken and filthy, they were nevertheless human beings. Moreover, this den of filth and wretchedness was gradually corrupting the atmosphere about: two respectable and temperate citizens residing in the vicinity had already died. Thus, the urgings of humanity, re-enforced by the instinct of self-preservation, left to the ordinary man no choice but to fight the epidemic as effectively as possible.[20]

To combat the epidemic, with drug and lancet, but perhaps more importantly with arguments and rationale, was the task of the physicians of New York. There was no Benjamin Rush in New York in 1832, there were no Snows or Sydenhams, but New York's four hundred and fifty regular practitioners were nevertheless a large and competent group, certainly in comparison with their medical contemporaries in the hinterlands.

Cholera was a manageable disease. Of this the regular physicians were assured. It could be deprived of its malignancy if the "premonitory" symptoms were treated in time; and it had been "proven" that a "painless diarrhea" was the universal premonitory symptom. Belief in the

[20] New York *Commercial Advertiser*, July 17, 1832.

efficacy of this principle of treatment—or some other—was a necessary and, perhaps, inevitable mechanism by means of which physicians and laymen alike preserved a measure of confidence when surrounded by uncertainty and death. Only those who had first predisposed themselves, and had then ignored the premonitory symptoms, became cholera victims. It is enlightening to read the description by a Scotch physician, widely read in New York, of his reaction upon first becoming acquainted with this principle.[21]

> . . . how rejoiced was I to find, after a single inspection of the hospitals, the disease managed upon a correct system, the medical attendants calm and philosophical, the indications of cure flowing out of premises easily to be understood; and all that was obscure, mysterious, and empirical . . . now dependent on rules of science easily comprehended.

The encouraging results which the physicians obtained were most likely due to the fact that they were treating people who did not have the cholera or who had at the worst a minor case. Every mild diarrhea treated by a physician was accounted a cure of cholera. But this position provided a necessary security. For if one avoided the predisposing causes, did not eat or drink to excess, kept warm, avoided over-excitement, and received treatment at the first signs of any bowel complaint, there was no reason to fear death from cholera.

Still, there were problems of therapy. How was the painless diarrhea to be treated? And what was to be done for those unfortunates who had progressed beyond the premonitory symptoms? Here it seemed that no two physicians could agree precisely; each practitioner had his own pet remedy or combination of remedies. A representative course of treatment was that recommended by the Special Medical Council of the Board of Health. They advised blood letting " to mitigate the spasms and render the system more susceptible to the action of the grand remedy, Mercury." The patient's skin was to be kept warm by continued rubbing with such substances as powdered chalk, cayenne pepper, mercury ointment, and calomel; he could be regarded as out of danger " when his mouth becomes sore or the discharges bilious, from the operation of mercury." [22]

Despite the controversy over prophylaxis and therapy, the medical

[21] James B. Kirk, *Practical Observations on Cholera Asphyxia* . . . (New York, 1832), p. 4. Kirk was very influential in New York and was credited with the discovery that a preliminary diarrhea always precedes the onset of cholera.

[22] N. Y. C. Board of Health, *Questions of the Board of Health, in Relation to the Malignant Cholera, with the Answers of the Special Medical Council* (New York, 1832), p. 5.

debate which generated the most emotion was that over the possibly contagious nature of cholera. If cholera were contagious, it would define the conduct of the whole community; how the hospitals were to be organized; what preventive measures were to be emphasized; and, most important, whether a quarantine was to be instituted—the last a measure which vitally concerned every man of business.

In 1832 few medical men believed that cholera was a contagious disease, though some hedged by suggesting that it might become so under particularly vile conditions. Even this halfhearted belief in "contingent contagion" had well night disappeared by 1849, when America next experienced a cholera epidemic. It is an ironic comment on changing patterns in medical thought that in the years preceding the discoveries of Koch and Pasteur the idea of contagion was, in most medical circles, in an exceedingly disreputable state. Professor Ackerknecht [28] has offered a perceptive explanation of some of the factors in this development. He describes contagionism at the opening of the nineteenth century as a seemingly medieval belief, one which had never been subjected to scientific scrutiny:

> It is no accident that so many leading anticontagionists were outstanding scientists. To them this was a fight for science, against outdated authorities and medieval mysticism; for observation and research against systems and speculation.

Furthermore, quarantines, logical result of a belief in contagion,

> meant to the rapidly growing classes of merchants and industrialists a source of losses, a limitation to expansion, a weapon of bureaucratic control that it was no longer willing to tolerate. . . . Contagionism would, through its association with the old bureaucratic powers, be suspect to all liberals trying to reduce state interference to a minimum.

There were many other, less abstract, reasons, some peculiar to the United States and others peculiar to cholera, which made almost inevitable the victory of anticontagionism in America in 1832.

Epidemiological thought in the United States had been conditioned by experience with yellow fever, and the "black vomit" was obviously noncontagious. Cholera, like yellow fever, has certain epidemiological anomalies, which are inexplicable on the basis of exclusive contagion. The consistent failure of quarantine, especially the *cordon sanitaire* between Russia, Poland, and Austria in 1831, put the burden of proof upon the

[28] Erwin H. Ackerknecht, "Anti-Contagionism between 1821 and 1887," *Bull. Hist. Med.*, 1948, *22*: 567.

contagonists, who were forced either to argue that the epidemic would have been worse without the quarantine, or to prove exactly how it had proved inadequate.

As was the case with yellow fever, so was it with cholera: there could never be found any pattern within the cases which would support a contagionist argument. Cholera always seemed to start *de novo* in widely separated parts of the city. Moreover, experience with smallpox—which everyone acknowledged to be truly a contagious disease—had convinced medical men that an attack of a *contagious* disease left one with a permanent immunity, certainly not the case in cholera which could be taken more than once.

To the socially conscious, forward looking physician, the doctrine of contagion was, in itself, an "exciting cause" of the disease. The fear which it generated would not only result in many additional cases, but would completely disrupt the social structure; cities would be deserted; commerce would cease; the sick would be left to die alone and without the simplest comforts.

The outbreak of cholera in Europe, in 1831, was the signal for American physicians to begin the debate over the necessity of quarantine. Prevailing medical opinion, as in Europe, was distinctly hostile. Dr. Joseph M. Smith, in a lecture on cholera delivered in November, 1831, argued that the disease was of aerial origin and, unlike yellow fever, could not be transmitted by *fomites*: quarantines could not affect it.[24] But there was such minority opposition to any precipitate discarding of quarantine regulations:

> . . . were the problem of the disease being contagious much less than it is, it would still be fairly worth considering, whether the removal of a probable or even possible source of infection to our whole population, were not worth a temporary inconvenience to a few individuals.[25]

By the time cholera had run its course in New York City, even this moderate position had become an intellectually indefensible one.

The failure of quarantine and the seemingly unpredictable pattern of cases during the epidemic brought complete victory for the anti-contagionists. "Non-contagion," urged that all those who furthered the "wicked doctrine" of contagion, "should forthwith be put *hors de combat*, or delivered over to the keeper of a cholera or insane asylum." One

[24] Joseph Mather Smith, *Discourse on the Epidemic Cholera Morbus* . . . (New York, 1831), pp. 22, 23.

[25] *Boston Medical and Surgical Journal*, 1832, *5*: 65.

cynical medical man declared that existing quarantines were the result of a yielding by the thinking part of the community to the irrational terrors of the panic-stricken multitude. He reflected that " some future historian will record our folly and credulity in the same chapter of events with Salem witchcraft, divining rods, and animal magnetism." [26]

But these are the enlightened opinions of the professional. The general belief among ordinary men was that cholera was caused by some specific contagion. It would have been unthinkable for the Mayor and the Governor not to have proclaimed rigid quarantine. A number of business men who petitioned the Board of Health against the establishment of a cholera hospital near their enterprises, argued that it was " vain to contend that the disease is not contagious, as the great body of our citizens are impressed with a dread of contact with the victims." As intelligent a man as Philip Hone could not doubt but that the cholera had been brought to Canada by the emigrants—an observation contravened by most medical men who had studied the problem.[27] A skepticism of accepted doctrine seems to have been particularly strong in the poor who, little impressed by the pleas of their betters, continued to find solace not in the Puritan regimen prescribed for them, but in their usual anodynes. Yet underlying the attitudes of almost all articulate New Yorkers was the web of belief which has been outlined.

Faith and reason, religion and science had been interwoven so as to provide a usable context in which to place the epidemic. No radical inconsistency lay between the doctrine of predisposing causes and that of retribution by the Lord. Theology was underwritten by the prestige of science and the data of science made palatable by a patina of moral satisfaction. An explanation was devised; one contrived to answer the present needs of a society and still conform to pre-existing patterns of belief.

[26] New York *Evening Post*, July 12, 1832; Christopher C. Yates, *Observations on the Epidemic now Prevailing in the City of New York* (New York, 1832), p. 34.

[27] Petition of G. D. Strong et al . . . addressed to the Board of Health, July 7, 1832. City Clerk's Papers, File Drawer U58, MARC; Hone, *Diary*, September 20, 1832, NYHS, Also in the Nevin's edition of the Hone *Diary*, vol. I, p. 78.

THE DILEMMA OF JOHANN JACOB WEPFER:

The History of the Contracted Kidney*

PAUL KLEMPERER** and WALTER FEDERN

In 1727, thirty-three years after his death, the grandsons of the great seventeenth century physician and scientist, Johann Jacob Wepfer, published a collection of medical observations [1] that he had assembled during his life. We have taken part of his ninth observation [2] as the point of departure for a survey of the evolution of present-day knowledge of the contracted kidney. The case report from the year 1681 carries the title: "Contusion of the head followed by fatal apoplexy." From Wepfer's records only data pertinent to the topic will be abstracted.

The patient, a 47 year old Count Fürstenberg, had not been in good health for several months. He complained of poor appetite and frequent vomiting, he suffered from blurred, occasional double vision, somnolence, and frequent speech difficulties; in addition to these symptoms preceding his fatal accident, it is stated that he had polyuria for more than two years and that the urine was always pale and thin. He had lost much strength and was emaciated. In spite of his ailing condition the Count decided to travel from his castle at Wolfach to Strasbourg to present himself to King Louis XIV whose army had occupied the city. On the two days' trip his symptoms continued. On the third day he planned to attend the triumphant entrance of the king. He fell down several steps, struck his head, became unconscious, and died within twenty hours.[3] Ten hours later a complete autopsy was performed by Wepfer.

From the detailed protocol it should first be mentioned that the cause of death was found in a hemorrhage into the left lateral, the third, and fourth ventricles of the brain.[4] Our main interest concerns the renal findings. The following description is given: The kidneys were remark-

* Presented at the thirty-first annual meeting of the American Association for the History of Medicine, New York City, May 22, 1958.

** Aided by a grant of the Dr. Eli Moschcowitz Fund.

[1] Wepfer, Johann Jacob: *Observationes medico-practicae, de affectibus capitis internis et externis.* Scaphusii, Joh. Adam Ziegler, 1727.

[2] *Ibid.*, pp. 13-23.

[3] The incident is mentioned, e. g., in Legrelle, Arsène: *Louis XIV et Strasbourg.* 4. ed. Paris, L. Hachette et Cie, 1884, p. 576.

[4] Wepfer, *op. cit.*, p. 18.

ably small and thin, in total mass less than those of a nine-year old boy. On the surface of both several hard nodules, larger than a pea, projected, one of which, the size of a grape, was cystic with a thick membrane which upon incision evacuated yellow serum.[5] This description seems to warrant the diagnosis of a granular contracted kidney.

It is remarkable that Wepfer intuitively grasped the pathophysiological problem presented by the renal abnormality, and he unequivocally states the basic question in the scholium, his clinicopathological discussion of the case, in the following paragraph:

> It is astonishing that the nobleman had already urinated very copiously for more than two years, if, in accordance with Bellini and Malpighi, the segregation of the serum (i. e., from the blood) takes place in the outer portion of the kidneys, since both their surfaces were occupied by hard and deep-seated nodules, and since the kidneys were also extremely small and slender. One would be inclined to believe that it had happened because of an excessive laxity of the canaliculi in the papillary carunculae and in the absence of any fermentation. This is suggested because of the slimness of the kidneys on the one hand, and because of the crudity of the urine on the other. Certainly the pelves and the ureters were extremely wide, although never a stone had been passed.[6]

The dilemma of Wepfer is the same which confronted physicians of the nineteenth century after Bright, the question of why and how an atrophic kidney excretes more dilute urine than the normal organ.

The quoted paragraph reveals the state of information and the concepts which prevailed at the end of the seventeenth century in regard to renal anatomy and physiology. It also illustrates an attempt of a scientific physician at correlating his observations at the bedside and at the autopsy table with the available data of these then basic sciences. It might be of interest to trace the evolution of observations and notions on the form and function of the kidneys from early records to the time of Wepfer.

The core of his comment is the affirmation that urine is serous fluid segregated from the blood by the kidneys. This belief is stated by most Greek writers, with the exception of Asclepiades, without source reference. The concept was most probably reached by correlation of observations

[5] *Ibid.*, p. 17.

[6] *Ibid.*, p. 20 (IV): Mirum Illustrissimum Dominum jam ultra biennium copiosissime semper minxisse, si seri segregatio fit in parte extima renum secundum Bellinum et Malpighum, cum utriusque superficiem occuparint tubercula dura, profunde infixa; cum etiam Renes mole sua admodum parvi et graciles fuerint: Credendum id factum fuisse propter canaliculorum laxitatem nimiam in carunculis papillaribus idque citra aliquam fermentationem tum ob exilitatem Renum, tum ob cruditatem urinae: Certe pelves et uretheres admodum ampli erant, quamvis nullum unquam Calculum excreverit.

of the priest at sacrificial ceremonies and of the butcher at the slaughter of animals, who noticed on the one hand the separation of yellow serum if blood coagulates, and on the other the connection of the kidneys with the bladder by the ureters. This supposition can be documented by passages in Hippocrates' *Peri sarkon*,[7] Plato's *Timaeus*,[8] Aristotle's *De partibus animalium*[9] and repeated statements of Galen.[10] This first phase of scientific theory of urine formation was followed by the question of how the segregation of the serum from the blood is accomplished. In the chapter "On the Kidney" in the Hippocratic book *On the Nature of Bone*,[11] it is maintained that the urine is filtered by spongy matter in the kidney and in the lower urinary channels. The period of origin of the book is uncertain. Rufus of Ephesus[12] and Aretaeus of Cappadocia[13] were probably the first to postulate the existence of a sieve (*ethmos*) to account for the separation. Both authors are vague as to its location, Rufus referring to the hilus, Aretaeus to the pelvis. The sieve analogy was accepted by Galen,[14] who localized it around the pelvis. After Galen, anatomic influence upon physiology waned, and for more than a thousand years there is no new reference to the sieve; but the problem of urine formation found a variant interpretation in Arabic medicine. According to Avicenna,[15] the kidney attracts the serum from the renal vein into its pelvis. In 1316 Mondino de'Luzzi[16] wrote his *Anathomia*, which remained the most widely used text for nearly 200 years. He took up the search for the site of the sieve and described in detail its location. According to him, the wall of the renal vein (emulgent vein) is per-

[7] Cap. 8 (ed. Kuehn, vol. I, p. 431). Littré, in his edition, vol. VIII, p. 595, translates: Lorsqu'on tue une victime, le sang, tant qu'il est chaud, est fluide, et il se coagule quand il se refroidit. Si on l'agite, il ne se coagule pas.

[8] 83 D; 85 D.

[9] II, 4 (650 b-651 a). Cf. *Historia animalium*, III, 6 (515 b); III, 19 (520 b).

[10] E. g., *De usu partium*, V, 5 (ed. Kuehn, vol. III, p. 363); urinae vi renum secernuntur a sanguine.—*De naturalibus facultatibus*, I, 13 (ed. Kuehn, vol. II, p. 31).

[11] *De ossium natura*, ed Kuehn, vol. I, p. 504 f.

[12] *De appellationibus partium corporis humani*: "De renibus, urinariisque meatibus" (ed. Daremberg-Ruelle, p. 181).

[13] *De causis et signis diuturnorum morborum*, II, 3 (ed. Kuehn, p. 134 f.); *De causis et signis acutorum morborum*, II, 9 (ed. Kuehn, p. 55).

[14] *Definitiones medicae*, 56 (ed. Kuehn, vol. XIX, p. 361 f.).

[15] *Canon medicinae*, III, 17, 1, 1. Transl. by P. de Koning in *Trois traités d'anatomie arabe*. Leide, E. J. Brill, 1903, pp. 736-738.

[16] Berengario da Carpi, Jacopo: *Commentaria cum amplissimis additionibus super anatomia Mundini una cum textu eiusdem in pristinum et verum nitorem redacto*. Bononiae, Hieronymus de Benedictis, 1521, fol. 158 b-159 a.

forated where it runs close to the renal pelvis and, through the foramina, serum dribbles into the pelvis. His observation was generally accepted, and Zerbi, one of Vesalius' predecessors in Padua, still adhered to it in his textbook published in 1502 referring to " that membrane (*panniculus*) by which water is separated from the blood." [17] Vesalius [18] published a

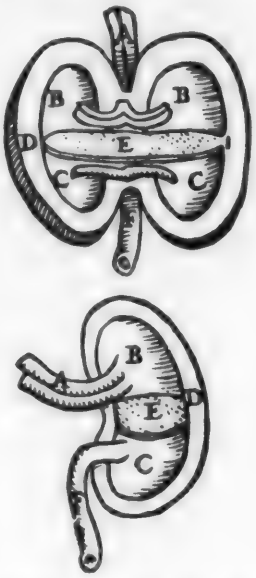

FIG. I. Vesalius (see footnote 18): The renal sieve as conceived by Zerbi.

schematic picture illustrating this spurious anatomical concept which he attacks in his usual violent manner. Berengario da Carpi overthrew Mondino's teaching when he published (1521) in his commentaries the results of his own investigations of the renal vein.[19] Since he could not discern a perforated wall with his own eyes, he injected warm water *per syringam* into its lumen and failed to perceive its escape from the ureter. But beyond refuting Mondino's erroneous notion, he observed that the entire kidney swelled up, and on incision he saw water escape upon the

[17] Zerbi, Gabriele: *Liber anathomie corporis humani et singulorum membrorum illius.* Venetiis, Bonetus Locatellus, 1502, p. 36.

[18] Vesalius, Andreas: *De humani corporis fabrica libri septem.* Basileae, J. Oporinus, 1555, p. 632.

[19] *Op. cit.*, (note 16) fol. 178 b-179 a.

outer surface. His observation that "he has seen the smallest veins proceed up to the outermost parts of the kidney" [20] was of revolutionizing novelty. It repudiated Aristotle's [21] idea of the limited ramification of the renal vessels and invalidated the belief that only the inner part of the kidney was of functional significance, an idea to some extent still shared by Vesalius [22] twenty years later when he maintained "that the vessels though aiming at the outer side of the kidney do not reach the surface, but remain within the internal body of the kidney."

It might be well to call special attention to Berengario's use of injection for his investigation more than one hundred and fifty years prior to the masters of the seventeenth century through whom the method contributed much to the advance of anatomy. The introduction of a hardening fluid for the demonstration of blood vessels in the human cadaver had been practiced by Alessandra Giliani,[23] a woman prosector of Mondino's. Thus Berengario found an established tradition in Bologna. But while injection had previously been used for didactic purposes only, it became a tool of research in Berengario's hand.

Of his immediate followers, Bartolomeo Eustachio deserves the highest credit. In 1563 he published his *Opuscula anatomica* which includes a 147-page treatise on the kidneys with six exquisite copper etchings.[24] This essay gives a detailed review of previous observations and ideas, and the results of his own investigations. He was the first who clearly described the renal calyces and their relation to the carunculae papillares with their foramina, already known to Berengario, through which urine escapes. He unequivocally assumed the existence of *sulci* and *canaliculi* in the renal substance, through which, he does not doubt, urine flows towards the *calyces*. While he concedes the difficulty of their demonstration, he deserves much of the credit which subsequent generations have given to Bellini (see Wepfer) who did not mention him in his work, *De structura renum*, published exactly one century later. Eustachio is best known for the discovery of the suprarenal glands, described and illustrated in his essay. Beyond these important additions to the knowledge of renal anatomy, Eustachio's book is fascinating for the modern reader because

[20] *Ibid.*, fol. 179 a.

[21] *Historia animalium*, I, 17 (496 b); *De partibus animalium*, III, 9 (671 b).

[22] *Op. cit.*, p. 633.

[23] Cf. Mead, Kate Campbell Hurd: *A History of Women in Medicine*. Haddam, Conn., Haddam Press, 1938, p. 225.

[24] Eustachio, Bartolomeo: *Opuscula anatomica*. Venetiis, Vincentius Luchinus, 1563, pp. 1-147: "De renum structura, officio, et administratione."

he finds some of the present day problems sagaciously discussed. Several chapters, for instance, deal with what one may today call the energy problem of urine formation. Of course, in the pre-Harveyan period the Galenic theory of attraction was the only possible explanation for the diversion to the kidneys of the serous superfluity of the blood. But the discussion of the question of whether this attraction is the same as that exerted by the magnet, as Averroes maintained, or whether it is a faculty of the organized substance, is quite reminiscent of the controversy between the mechanistic theory of Carl Ludwig and the vitalistic concept of Heidenhain.

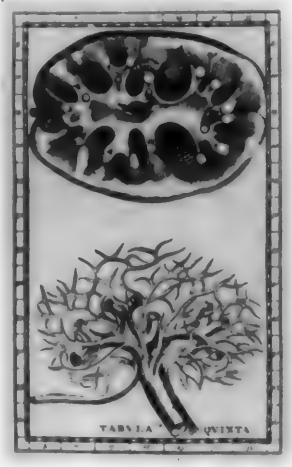

FIG. II. Eustachio (see footnote 24), table V: Bisected kidney and ramifications of vessels and renal pelvis.

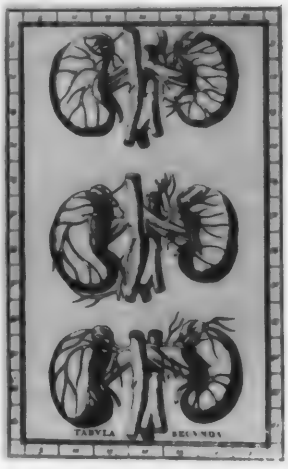

FIG. III. Eustachio (see footnote 24), table II: Blood supply to kidneys and adrenal glands.

Mondino's notion of a sieve located in the wall of the renal vein had been found false by Berengario's exact investigation, but the mechanism by which separation of the serum was achieved was ascertained neither by him, nor by Eustachio or Falloppio.[25] Since it was established that blood circulates throughout the entire kidney, it was logical to assume that the whole organ might serve this end. The gross analogy with a sieve right back of the urinary reservoir of the pelvis had lost its validity.

[25] Falloppio, Gabriele: *Observationes anatomicae.* Venetiis, Marcus Antonius Ulmus, 1561, p. 179.

But a filtering device back of the finer urinary conduits was a reasonable alternative. This was a promising possibility and thus we again encounter, as in ancient times, a postulate which had to be supported by actual observation. Eustachio and Falloppio had searched for urinary channels, but it remained for the nineteen year old Bellini to demonstrate with skillful tissue dissection, injection, and with the aid of the microscope, that " the substance of the kidneys is nothing else than an aggregate of countless vessels *sui generis* " and " that the filaments are continuous from the outer surface to the pelvis." [26] With the structure of the kidney

FIG. IV. Bellini (see footnote 26) fig. 10: Deer's papilla.

established, Bellini tried to account for the separation of the serum from the blood. His explanation which closely follows that of his teacher, Borelli,[27] is untenable today because Borelli astonishingly was unaware of the capillary connections between arterial and venous ramifications. According to the pre-Malpighian concept, he believed that the blood escapes through arterial openings into the substance of the kidney. The free blood " now meets two types of vessels; one venous, the other renal. The serum enters the renal ducts separated from the blood, the blood

[26] Bellini, Lorenzo: *Exercitatio anatomica de structura et usu renum.* Amstelodami, Andreas Frisius, 1665, p. 64.

[27] Cf. Borelli, Giovanni Alfonso: *De renum usu judicium.* Argentorati, Simon Paulli, 1664.—Idem: *De motu animalium.* 2. ed. Lugduni in Batavis, Petrus vander Aa et al., 1685, vol. II, ch. 9: " De sanguinis expurgatione in renibus," pp. 195-216, especially pp. 207 and 211.

the venous channels separated from the serous humor." [28] Borelli considers the possibility that the segregation is determined by the different caliber of the two types of channels but rejects it. He compares the inflow of serum with the rise of water in capillary tubes and alludes to the mechanism by which mercury penetrates gold, while neither water, nor air, nor any other finely dispersed material, can enter it.[29] One might well regard his ideas as adumbrations of the physico-chemical theories of renal filtration of the nineteenth and twentieth centuries. They reveal a far higher level of scientific integration than the ancient naive sieve postulate.

Four years after Bellini, Malpighi [30] discovered the renal glomeruli. He confirmed the existence of the uriniferous tubules as "wormlike bodies" [31] and described "the innumerable very small glands attached like apples to the blood vessels," [32] which secrete the urine. The idea that the kidneys are glands had already been entertained in antiquity; but it had not been a fertile concept. In that period glands had generally been defined as structures capable of removing superfluous humidity, and consequently the kidneys were included. No unifying anatomical principle was known, and Galen's main rationale for regarding the kidneys as glands was identity in taste. Malpighi's concept of glands was far clearer. He had recognized excretory ducts, he had seen the ultimate tubular structures of the testes in vertebrates as well as in invertebrates. He regarded the uriniferous canaliculi as excretory ducts and postulated a connection with the glandular glomeruli with the words: "I have worked a long time to make evident to the eye this connection which reason sufficiently attained." [33] But here he failed, and it remained for William Bowman, 176 years later, to reach his goal. Malpighi tried to comprehend the mechanism by which the glomeruli segregate the urine, to find "minimum illud organicum," [34] this smallest engine which is responsible for so important effects. "This engine," he contemplates,

[28] Bellini, *op. cit.*, p. 75 f.
[29] *Ibid.*, p. 79.
[30] Malpighi, Marcello: *De viscerum structura exercitatio anatomica.* Londini, Jo. Martyn, 1669, pp. 72-103: "De renibus."—Cf. Hayman, J. M., Jr.: Malpighi's "Concerning the structure of the kidney," a translation and introduction. In *Annals of Medical History*, 1925, *7*: 242-263.
[31] Malpighi, *op. cit.*, p. 78 (Ch. I).
[32] *Ibid.*, p. 83 f. (Ch. II).
[33] *Ibid.*, p. 87 (Ch. III).
[34] *Ibid.*, p. 135 (*De liene*, Ch. VI).

" must of necessity perform the work of separation by virtue of its internal configuration. But it is open to doubt whether this structure corresponds to those contrivances which we apply generally for human use. Admittedly there are analogous formations of sponges, sieves, and perforated tubes, but it is most difficult to say to which of these the renal structure is similar in all respects." [35] He doubts, in fact, that the renal structure is sufficient to accomplish adequate urine secretion and believes that " therefore a constant fermentation of the blood contributes to the separation of the urine, whereby the dissolved and free salt, as well as water particles are more easily removed by the existing structure and separated from the residue, and thereby the excretion and quality of the urine undergoes constant changes." [36] He certainly was aware of the complexities of renal physiology and understood that mere analogy was no adequate explanation. This was the information available to Wepfer when he tried to understand polyuria in terms of anatomic alteration.

Polyuria as a symptom of a disease termed diabetes [37] was first described by Aretaeus; [38] it was also known to Galen.[39] His explanation of the pathogenesis was mere speculation, invoking increased attraction of serous fluid and a weakness of retaining it as the cause of the disease. No autopsy reports were available at the time of Wepfer, as a perusal of Bonet [40] and Morgagni [41] reveals. Granular atrophic kidneys likewise had only occasionally been mentioned and the few reports give no adequate clinical data. Eustachio [42] casually refers to kidneys with surface tubercles and in his final pathologico-anatomic chapter, to similar abnormalities in apoplectics. Saliceto's famous reference to hardened kidneys is not quite pertinent to this discussion because it is associated with oliguria and ascites. However, he seems to have known of renal atrophy with pale and copious urine.[43] Whether he derived his information from autopsies cannot be decided

[35] *Ibid.*, p. 98 (Ch. VI).

[36] *Ibid.*, p. 100 (Ch. VI).

[37] Cf. Baumann, E. D.: De diabete antiquo. In *Janus*, 1933, *37*: 257-270.

[38] *De causis et signis diuturnorum morborum*, II, 2 (ed. Kuehn, pp. 131-134); *De curatione morborum diuturnorum*, II, 2 (ed. Kuehn, p. 329 f.).

[39] *De locis affectis*, VI, 3 (ed. Kuehn, vol. VIII, p. 394).

[40] Bonet, Théophile: *Sepulchretum, sive anatomia practica*. Genevae, L. Chouët, 1679.

[41] Morgagni, Giovanni Battista: *De sedibus et causis morborum per anatomen indagatis*. Venetiis, Typographia Remondiniana, 1761.

[42] *Op. cit.*, (note 24), pp. 134, 143, 144.

[43] Loewy, Eugen: *Beiträge zur Kenntnis und Würdigung Wilhelm's von Saliceto (XIII. Jahrh.) als Arzt*. Inaugural-Dissertation. Berlin, Gustav Schade (Otto Franck), 1897, p. 29.

from his writings. Certainly he was not original, because Serapion, the elder, 802-849, makes similar statements.[44] Balthasar Timaeus von Güldenklee found small kidneys, the size of a walnut, at autopsy of an old man with ascites.[45] Wepfer's is therefore the first clear description of salient clinical and pathologico-anatomic features of the contracted kidney. It is an evidence of his independent mind that he recognized the discrepancy between the symptom of polyuria and the renal atrophy. He was the first to pose the problem which even today has not been completely solved. It is surprising that the significance of his observation and of his question was not understood, either by his contemporaries, or by those who followed him, for more than a hundred years. This neglect is evident because neither Morgagni,[46] Lieutaud,[47] Rayer,[48] nor anybody else, makes reference to his recorded case. This omission may be explained by the title which did not indicate the existence of a renal abnormality.

Sir Charles Sherrington in his *Endeavour of Jean Fernel* says: "Essential to a great discoverer in any field of Nature would seem an intuitive flair for raising the right question." [49] According to this definition Wepfer deserves full recognition. But Sherrington continues: "To ask something which the time is not yet ripe to answer is of small avail." Obviously the knowledge of anatomy and physiology available at the end of the seventeenth century was not adequate to answer Wepfer's dilemma. It seems, therefore, of interest to inquire into the development of subsequent years, to understand when and how the state of information regarding renal form and function became sufficient to cope with the problem.

Malpighi's discovery of glomeruli and his ideas regarding their function found no unequivocal acceptance. Littre [50] in 1705, in a report of a polycystic kidney of a new-born baby, associated with an occlusion of

[44] Haeser, Heinrich: Zur Geschichte der Bright'schen Krankheit. In *Janus, Zeitschrift für Geschichte und Literatur der Medicin*, 1848, *3*: 371-72.

[45] Bonet, *op. cit.*, lib. III, sect. XXI, Obs. VIII, p. 1073.

[46] *Op. cit.*, (note 41).

[47] Lieutaud, Joseph: *Historia anatomico-medica.* Parisiis, Vincent, 1767.

[48] Rayer, Pierre-François-Olive: *Traité des maladies des reins et des altérations de la sécrétion urinaire.* Paris, J. B. Baillière, 1837-41.

[49] Sherrington, Sir Charles Scott: *The Endeavour of Jean Fernel.* Cambridge, University Press, 1946, p. 142.

[50] Littre, Alexis: Observation sur les reins d'un foetus humain de neuf mois. In *Histoire de l'Académie Royale des Sciences*, 1705 (Paris, Jean Boudot, 1706): Section "Mémoires," pp. 111-17.

the upper ureters, sided with Malpighi by interpreting the serum-filled cysts as distended glands. But his view was not accepted by subsequent authors, who regarded his case as a monstrosity far from illuminating normal anatomy. Frederik Ruysch,[51] one of the leading anatomists of the late seventeenth and early eighteenth centuries, contested Malpighi's theory. While he recognized the glomeruli, he regarded them as aggregations of small vessels designed to retard the blood circulation, and

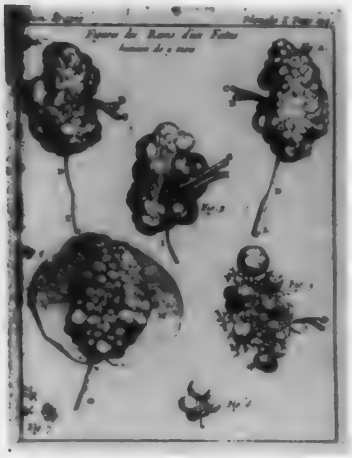

FIG. V. Littre (see footnote 50), fig. 6: Isolated cysts interpreted as cystic glomeruli.

FIG. VI. Ruysch, *Thesaurus anatomicus sextus* (Amsterdam 1744), Prodromus, fig. 2: Injected kidney.

not as secretory glands. In injected kidneys he saw continuity between blood and uriniferous channels and believed that the separation of the urine was determined by narrowing of the conduits. Boerhaave[52] accepted Malpighi's concept as well as Ruysch's and thought that the glomeruli were responsible for the secretion of inspissated urine. Albrecht von Haller[53] sided with Ruysch. Thus in the middle of the eighteenth cen-

[51] Ruysch, Frederik: *Thesaurus magnus et regius qui est decimus thesaurorum anatomicorum.* Amstelodami, Janssonio-Waesbergii, 1729, No. LXXXVI (p. 18 f.).

[52] Boerhaave, Hermann: *Institutiones medicae in usus annuae exercitationis domesticos digestae.* Editio Leydensis quinta prioribus longe auctior. Lugduni Batavorum, Theodorus Haak et al., 1734, n. 353 (p. 191 f.).

[53] Haller, Albrecht von: *Elementa physiologiae corporis humani.* Tomus septimus. Bernae, Societas Typographica, 1765, p. 275.

tury, in spite of much detailed work on the structure of the kidney, including comparative anatomy, the problem of urine formation was still in a confused state. Subsequently no progress was made in the question of glomerular function, but information regarding the form and function of the uriniferous canaliculi advanced through the discovery of the cortical serpentine tubules by Ferrein [54] in 1749 and his disclosure of the circumvening capillary network. His findings, completed more than a century later by Henle's [55] discovery of the loops, secured the present-day knowledge of the tubular portion of the nephron and its reabsorptive and secretory role in urine formation. Yet the question of the connection between glomeruli and uriniferous tubules remained obscure. The breakthrough came in the Strasbourg doctoral dissertation of a young Russian student, Alexander Schumlansky.[56] With painstaking zeal he followed under the microscope the progress of insufflated air in the uriniferous tubules, until he was convinced that they ended at the glomerular capsule. The appended figure, however, gave only an incomplete picture of what he described in detail in his thesis, and in consequence he did not receive full credit for his discovery. In fact, the inaccurate proportions of the depicted glomeruli in comparison with the width of the tubules led Huschke [57] and Johannes Müller [58] to doubt the verity of his observations. These two German authors were the last to refuse to accept the role of the glomeruli in urine formation, which they explained as the sole result of an interaction between convoluted tubules and the surrounding capillary network. Joseph Berres [59] clarified with elegant injections and superb illustrations the rete mirabile of the glomerular capillaries. But it was

[54] Ferrein, Antoine: Sur la structure des viscères nommés glanduleux, et particulièrement sur celle des reins et du foie. In *Histoire de l'Académie Royale des Sciences*, 1749 (Paris, Imprimerie Royale, 1753): Section, "Mémoires," pp. 489-530, especially p. 503 f.

[55] Henle, Friedrich Gustav Jacob: Zur Anatomie der Niere. In *Abhandlungen der königlichen Gesellschaft der Wissenschaften zu Göttingen*, Zehnter Band. Von den Jahren, 1861, 1862. (Göttingen, Dieterichsche Buchhandlung, 1862). Abhandlungen der physikalischen Classe, pp. 223-254.

[56] Schumlansky, Alexander: *Dissertatio inauguralis anatomica de structura renum.* Argentorati, Lorenz & Schuler, 1782.

[57] Huschke, Emil: Über die Textur der Nieren. In *Isis oder encyclopädische Zeitung*, 1828, *21*: 560-569.

[58] Müller, Johannes: *De glandularum secernentium structura penitiori earumque prima formatione in homine atque animalibus.* Commentatio anatomica. Lipsiae, Leopold Voss, 1830.—*See* Solly, Samuel: *The Intimate Structure of Secreting Glands, by John Müller, with the Subsequent Discoveries of Other Authors.* London, Joseph Butler, 1839.

[59] Berres, Joseph: *Anatomie der mikroskopischen Gebilde des menschlichen Körpers.* Wien, Carl Gerold, 1837, Tafel X, fig. 2.

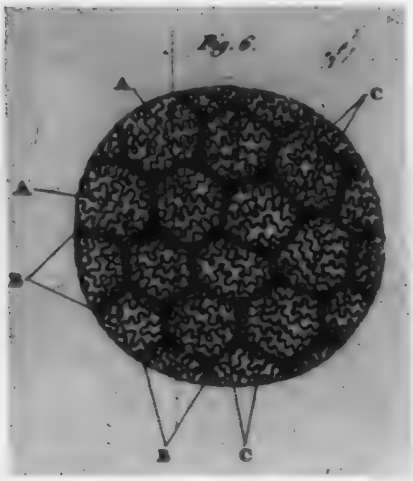

FIG. VII. Ferrein (see footnote 54), *planche* 3, fig. 6: Surface of bird's kidney, magn. 7x.

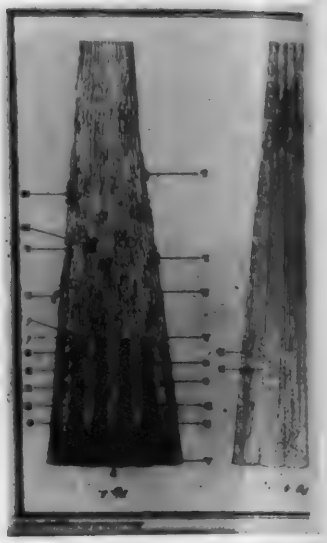

FIG. VIII. Ferrein (see footnote 54), *planche* 3, fig. 5: Human kidney serpentine cortical and straight medullary uriniferous tubes, magn. 10x.

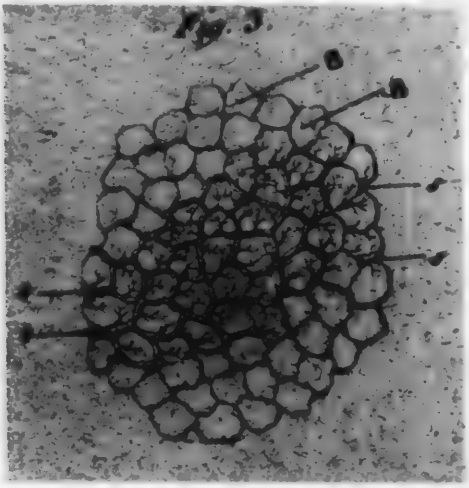

FIG. IX. Ferrein (see footnote 54), *planche* 1, fig. 3: Surface of human kidney, Loup magnification. Vascular network and capillaries.

William Bowman [60] who in 1842, at the age of twenty-four, unequivocally confirmed Malpighi's rational conjecture. The precision of observation and the clarity of his physiologic conclusions climax all the efforts of preceding centuries to comprehend the structure and function of the kidney. It is sufficient to quote just two short sentences from his immortal work: "The Malpighian bodies might be an apparatus destined to separate from the blood the watery portion," and "The membrane of the

FIG. X. Huschke (see footnote 57): a. convoluted tubules, b. capillary network.

uriniferous tubes corresponds with that forming the secreting surface of other glands." [61] Here are united in perfect harmony the ancient two leitmotivs: the kidney a sieve, the kidney a gland. Bowman's contribution was the final chapter in what may be called physiological anatomy of the kidney. The further advance in our knowledge of renal functions is dominated by research in biochemistry and experimental medicine, always with due regard for the foundations of structure which had been laid by the efforts of preceding centuries. The story of this progress need not

[60] Bowman, William: On the Structure and Use of the Malpighian Bodies of the Kidney, with Observations on the Circulation through that Gland. In *Philosophical Transactions of the Royal Society of London*, 1842, *132*: 57-80.

[61] *Ibid.*, p. 73.

be told; it can be found in broad outline in every physiological text, in detail in Cushny's [62] *Secretion of the Urine* and in Homer Smith's Kansas *Lectures of the Kidney.*[63]

A few remarks may be in order to complete the history of the con-

FIG. XI. Berres (see footnote 59), *Tafel* X, fig. 2: Injected glomeruli.

tracted kidney. After Wepfer no significant observations were made for for more than a hundred years. In fact, concern with renal atrophy begins only with Richard Bright,[64] although he did not fully share Wepfer's dilemma because he did not pay adequate attention to polyuria

[62] Cushny, Arthur Robertson: *The Secretion of the Urine.* London etc., Longmans, Green & Co., 1917. (Monographs on Physiology).

[63] Smith, Homer William: Renal Physiology between Two Wars. In his *Lectures on the Kidney.* Lawrence, Kansas, University Extension Division, University of Kansas, 1943, pp. 65-82.

[64] Bright, Richard: *Reports of Medical Cases Selected with a View of Illustrating the*

and isosthenuria. But he described granular kidneys and recorded a copious urine of low specific gravity in some of his cases. Christison [63] of Edinburgh, in 1839, stressed polyuria in the late stages of Bright's disease with occasional absence of dropsy and albuminuria. His observa-

FIG. XII. Bowman (see footnote 60).

FIG. XIII. Bowman (see footnote 60).

tions already implied existence of pathogenetically different forms of renal contraction. Microscopic studies led to the separation of the secondary inflammatory and the primary vascular contracted kidney. The idea that chronic pyelonephritis may also be responsible for granular renal atrophy

Symptoms and Cure of Diseases by a Reference to Morbid Anatomy. London, Longman et al., 1827.

[63] Christison, Robert: *On Granular Degeneration of the Kidneys, and its Connection with Dropsy, Inflammations, and Other Diseases.* Philadelphia, A. Waldie & Co., 1839, p. 25.

was first introduced by Liebermeister [66] and Bartels [67] in the nineteenth century, further elaborated by Löhlein,[68] and Parker and Weiss,[69] in the twentieth. The polyuria in spite of atrophy of renal parenchyma was first explained by Traube [70] and Bartels [71] as the result of increased glomerular filtration, engendered by compensatory vascular hypertension, in recent times by diminished reabsorption of water at the tubular level; yet even today the problem has not been completely resolved. Exact localization in the nephron of the reabsorptive defect demands further clarification. Studies of renal function with the methods of Homer Smith,[72] correlated with quantitative investigations of tubular morphology in cases of contracted kidney, as introduced by Jean Oliver,[73] may ultimately lead to an unequivocal answer to Wepfer's dilemma.

If one surveys the slow unveiling over the centuries of the form and function of the kidneys, one does not know what to admire more; the sagacity of a Wepfer, the divination of a Malpighi, the scientific accuracy of a Bowman, or the wondrous disclosures of our time represented by the work of Richards,[74] Smith, and Oliver.

The authors express their appreciation to Mrs. Alice D. Weaver and staff of the New York Academy of Medicine's Rare Book Division for the many courtesies which aided in the preparation of this article.

[66] Liebermeister, Carl von: *Beiträge zur pathologischen Anatomie und Klinik der Leberkrankheiten.* Tübingen, H. Laupp & Siebeck, 1864, p. 75.

[67] Bartels, Carl: Die allgemeine Symptomatologie der Nierenkrankheiten und die diffusen Erkrankungen der Nieren. (*Handbuch der speciellen Pathologie und Therapie*, hrsgegeben von Hugo von Ziemssen, vol. 9, pt. 1). Leipzig, F. C. W. Vogel, 1875, p. 375 f.

[68] Löhlein, Max: Über Schrumpfnieren. *Beiträge zur pathologischen Anatomie und zur allgemeinen Pathologie*, 1916/17, *63*: 570-632.

[69] Weiss, Soma, and Parker, Frederic: Pyelonephritis: its relation to vascular lesions and to arterial hypertension. *Medicine*, 1939, *18*: 221-315.

[70] Traube, Ludwig: *Über den Zusammenhang von Herz- und Nieren-Krankheiten.* Berlin, August Hirschwald, 1856.

[71] *Op. cit.*, (note 67).

[72] Smith, Homer William: *The Kidney; Structure and Function in Health and Disease.* New York, Oxford University Press, 1951.

[73] Oliver, Jean: An essay toward a dynamic morphology of the mammalian nephron. *The American Journal of Medicine*, 1950, *9*: 88-101.

[74] Richards, Alfred Newton: Urine formation in the amphibian kidney. *Harvey Lectures*, 1934-35, series XXX, Baltimore, The Williams & Wilkins Company, 1936, pp. 93-118.

THE NATIONAL INSTITUTES OF HEALTH RESEARCH GRANT PROGRAM AND THE HISTORY AND SOCIOCULTURAL ASPECTS OF MEDICINE *

PHILIP SAPIR AND JEANNE BRAND

The last several years have seen the development at the National Institutes of Health of a small but growing interest in the history and sociocultural aspects of medicine. Within the same period—in May, 1957—the American Association for the History of Medicine appointed a committee to keep the Association informed of any plans that might be made to establish an " Institute of the History of Medicine " at N. I. H. This action of the Association was in part predicated on the hope of some of its members that since the National Library of Medicine, the world's leading medical library, was to move to Bethesda an Institute in the history of medicine, similar to the other National Institutes of Health, might serve as a focus for coordinating and stimulating research and training in this area.

It is fairly clear, however, that the likelihood of establishing a separate Institute for this activity is slight. It is difficult to justify the need for such an Institute in view of the fact that the present administrative organization is adequate to provide assistance in this area as a part of the regular program activities of the National Institutes of Health. At the present time, also, the National Library of Medicine does not support the establishment of a separate Institute in the history of medicine.

It seems appropriate at this meeting, however, to give you some of the background to the interest of the National Institutes of Health in this field, as well as information on the extent and limitations of that interest.

The Role of the National Advisory Mental Health Council

As many of you know, the Public Health Service Act, which authorizes the grant and award programs of the Public Health Service, provides that the Surgeon General of the Public Health Service shall " encourage, cooperate with, and render assistance to appropriate public authorities, scientific institutions and scientists in the conduct and coordination of

* Presented at the thirty-first annual meeting of the American Association for the History of Medicine, New York, May 23, 1958.

research, investigations, demonstrations, and studies relating to the cause, diagnosis, treatment, control, and prevention of the physical and mental diseases and impairments of man."

No research or training grant may be approved by the Surgeon General unless it is recommended for approval by one of the National Advisory Councils. Members of these Councils, of which the National Advisory Mental Heath Council is one, also advise and make recommendations on program development.

In February 1956, the Council which advises the National Institute of Mental Health suggested that this Institute should extend its support beyond the clinical and laboratory studies which make up the bulk of its program, to the support of what might be called scholarly research activities. In June 1956, the National Advisory Mental Health Council further recommended that the National Institute of Mental Health give attention to supporting research and training in the history of psychiatry —broadly interpreted. The Council at this meeting noted the value of the historical and philosophical aspects of science and the need for stimulating a greater interest in the cultural and social context within which psychiatry and medicine are practiced. Subsequently, the Council, still concerned with the need for program development in the history of medicine, suggested that the Institute explore the possibility of creating a peripatetic lectureship in the history of science and medicine.

At this point, before taking action on the Council's recommendation, it seemed appropriate to the administration of the National Institute of Mental Health to explore the needs and status of this field. The result was a report presented to the Council by Dr. Jeanne Brand of the Institute's staff in June 1957. Dr. Brand's report, based on interviews with about 35 specialists in this field, indicated first, a need for additional trained specialists—people equipped to interpret both medicine and history to students. Secondly, the report pointed up a need for increased and improved teaching of medical history in medical schools; thirdly, a need for research support for the good scholar; and finally, modest assistance for supporting what might be called the tools of scholarship in medical history. Included in this were support for the translating, editing, and reproduction of early medical writings; support for visual aids in medical history; and funds for cataloging exceptional collections, of use to the scholar in future studies in medical history.

The Council, in accepting Dr. Brand's report (which appears as an appendix to this paper), reaffirmed its support for research and training in the history, philosophy and sociocultural aspects of medicine, and

requested the Institute to transmit to the other National Advisory Councils an invitation to join in support for this field. The Council also suggested that the Institute staff explore the possibility of holding a conference of medical historians and medical educators.

Support for the History of Medicine to Date

As you all doubtless realize, the National Institutes of Health for the most part dispense what are termed "categorical" grant funds. The major focus of the National Institute of Mental Health research and training grant programs is the promotion of mental health and cure of mental illness. Today we know, however, that mental health is intimately bound up with an understanding of how man operates in his society. We also recognize that the role of the medical man, to be most effective, must be practiced by persons who are able to see a patient in the context of his society, his socioeconomic needs, and in a broad perspective of past factors operating in the present. It is in keeping with this aim that the National Institute of Mental Health has been willing to put a modest share of its categorical funds into the support of broad-scale studies in the history and sociocultural aspects of medicine, as well as in historical studies particularly related to its categorical field of interest.

Up to the present, five research grants and two predoctoral fellowships have been awarded by the Institute in the general area of history of medicine. Dr. Ilza Veith of the University of Chicago and Dr. Gerald J. Gruman of Harvard University have received earlier research grant awards. Most recently, Dr. Eric T. Carlson of Cornell University Medical School and Doctors Gregory Zilboorg and George Mora have been awarded research grants for studies in the history of psychiatry. Dr. Richard Shryock is presently serving as a consultant to the Institute in this program, and several other members of the American Association for the History of Medicine have assisted in serving as members of ad hoc committees to review grant applications prior to Council consideration.

We are still in an exploratory stage as far as developing any program of departmental support for training in the history of medicine. Before the conference recommended by the Council can be convened we need to know more, among other factors, about the attitude of deans of medical schools to this field.

Future Applications and the Other Institutes

In coming before the Association today, we wished to review the historical background of our interest in this field. If any of you wish to apply for grant support a word of caution should be added. The National Advisory Mental Health Council has expressed itself as interested in broadly conceived studies in this field carried out by able scholars. There is, naturally, because of our categorical interests, a limit to the number of such projects which might be eligible for grant support. We do not yet know if the other National Institutes of Health will be willing or able to support studies in the history of medicine which may fall within their particular categorical interests. Several Councils have expressed interest in the field (the National Advisory Cancer Council and the National Advisory Neurological Diseases and Blindness Council, for example), but they wish to see the ideas to be implemented before they agree to commit any funds. Other Councils have expressed an interest but are unlikely to have funds in the near future to support this field. We do not wish to encourage you unduly to apply, without real hope of support, as has been indicated earlier. Although there is a limit to the amount of support the National Institutes of Health is likely to provide, well developed research applications will be given careful consideration.

The Nature of Grant Support

Research grant funds may be utilized for personnel costs, supplies, equipment and necessary travel, but only in the context of a well thought out and meaningful research proposal. The Public Health Service does not set minimum or maximum limits to the amount of its grants. Grant support may range from one to five years, occasionally longer. Grant applications in the history of medicine are presently reviewed by ad hoc committees of non-government experts, prior to consideration by the National Advisory Council of the Institute to which they are assigned. The use of specialized ad hoc committees is a practice often used with applications of special types. Permanent " Study Sections " for review of applications in any field are only set up when justified by the workload of applications.

At present, support for training in the history of medicine is limited to applications from individuals at the predoctoral and postdoctoral levels through the National Institutes of Health research fellowships program.

As historians you know that the search into the past provides new perspectives and understanding of the present. Medicine and the history

of developing social responsibility in medical care are only a part of the great panorama of history which has led to the present. If such knowledge can widen our understanding of man in his environment—can bring a greater depth to the skills with which the physician attempts to cure the sick—can make us reassess present treatment methods against the double past of the history of medicine and the history of the patient in his sociocultural milieu, it is worthy of aid from society.

APPENDIX

REPORT TO THE NATIONAL ADVISORY MENTAL HEALTH COUNCIL ON THE HISTORY OF MEDICINE

May 31, 1957

The Present Issues

During its June 1956 and February 1957 meetings, the National Advisory Mental Health Council has given consideration to encouraging further development in the history of psychiatry and in the history of medicine generally.[1] The Council has recognized a need for broader horizons in medical research and education and has affirmed the value of nurturing in the investigator an attitude of mind in which historical and philosophical perspectives can fertilize current research. While support for a cultural understanding of medicine is particularly appropriate from a national advisory body dealing with mental health, the history of medicine is also a field which crosses the interest of all the National Institutes of Health. A number of Institutes have in the past supported historical projects which had particular application to their respective work. It is unclear at present, however, which Institutes would support research and training in the general area of the history of medicine. In addition, no present review mechanism

[1] Specifically, at the June 1956 meeting, the Council passed a motion requesting the staff at N.I.M.H. to explore the possibilities of finding appropriate places in which support could be given to further development of [the] body of knowledge and its teaching in the history of psychiatry—broadly interpreted. In the discussions at the meeting, interest was expressed in having the National Institutes of Health recognize the importance of encouraging activity in the history of medicine generally. . . . At the February 1957 meeting, the Council passed a motion requesting the staff of the National Institute of Mental Health to explore with the administration of the N.I.H., and perhaps other Institutes, the possibility of creating a peripatetic lectureship in the history of science and the history of medicine . . . with the concept of relating it somehow to the National Library of Medicine.

at the National Institutes of Health provides for professional evaluation of grant applications by medical historians.

If the Council wishes to reaffirm its support of program development in this area, the Council might wish to consider the formulation of a resolution to be transmitted to the other National Advisory Councils and to the administration of the National Institutes of Health requesting they join with the National Institute of Mental Health in supporting research and training in the history, philosophy, and cultural aspects of medicine.

As the history of medicine is a discipline with rather specialized problems, the following brief survey, compiled in part from a series of discussions with leading U. S. medical historians, is submitted for the Council's consideration to facilitate review.

Current Problems in the History of Medicine

A. *The Need for Trained Specialists and Teaching Opportunities.*

A majority of medical historians consulted agreed that an outstanding need in the field today is for the training of a small, capable corps of specialists. Two classes of people are teaching and writing medical history at present: first, the "talented amateur"—most often a physician who has taken up the history of his field as a hobby; secondly, the professional—either an historian who has had some additional training in medicine or science, or a physician with graduate training in the historical method. Both the amateur and the professional have contributions to make.

Few persons in this country, however, have embarked upon the long training necessary in both fields, as, until recently, job opportunities have been severely limited. Teaching posts in the history of medicine *per se* are still very few in number,[2] and until more medical schools and universities are willing to allot the money and time for courses in this area, the career placement of its specialists will continue to be difficult.

B. *The Need for Increased and Improved Teaching of Medical History in Medical Schools.*

While there are today more scholars, more "departments," more "chairs," and more publications in the history of medicine than existed a century ago, the average medical student and practitioner know less of the history of their profession today than their predecessors of a hundred years ago.[3] As science has become less authoritarian, its older "authorities" have been forgotten. Medical history has been shunted aside as an antiquarian hobby for which a crowded curriculum found little time. Yet, as the Committee on the Teaching of Medical History pointed out:

---

[2] A survey of the teaching of medical history, undertaken in 1951 by the American Association of the History of Medicine, indicated that 37 out of 79 medical schools in the United States offered instruction, through organized courses, in the history of medicine. [*Bull. Hist. Med.*, 1952, *26*, 574-578.] There are probably fewer than fifteen full-time, professional teaching positions in the history of medicine in the United States today.

[3] Report of the Committee on the Teaching of Medical History, *Bull. Hist. Med.*, 1951 *25*, 571-572.

> In the wide expanse of the present medical curriculum there are few if any areas, other than that of medical history, in which students are encouraged to step back from the technical specialties and view their science and their profession in terms of larger interrelationship—of both those within medicine and with society as a whole.[4]

Many of those who have taught medical history as an avocation have been unable to offer satisfactory instruction as they lack historical training and are unfamiliar with advances in historical development—particularly in social and intellectual history.

C. *The Need for Research Support for the Mature Scholar.*

The mature medical historian has amassed a body of professional and life experience which brings depth both of interpretation and understanding to his research and writing. Yet there is relatively little research support available for the physician-scholar, earning his livelihood in medical practice, to help finance the writing in the history, philosophy, and cultural aspects of medicine. The proven formula of "Find a good man, give him financial support, and let him go ahead on his own" has as much application to research in the history of medicine as it does in other fields.

D. *The Need for Supporting the Tools of Scholarship in Medical History.*

As the laboratory is to the experimental scientist, so the library is to the historian. Among some current needs in the history of medicine are modest support for the translation, editing, and reproduction of early medical writers; funds for scholars to carry out research at outstanding medical libraries; modest support for appropriate visual media; and funds for cataloguing exceptional collections, available to the public, in medical history.

The Value of Support in the History of Medicine

The Council has already taken note of the present-day fragmentation of science—of the great technical and professional orientation of research investigation—and of the need to broaden the scientist's outlook on his social environment. Historical and philosophical perspectives on contemporary research can also furnish new starting points for investigation, can help to engender that scholarly breadth necessary to the basic growth of ideas. There are, too, practical results in the historical review of medical practice. Cocaine, quinine, and rauwolfia serpentina, for example, stand among the "rediscovered remedies" in modern medicine.

Medical history also helps to provide the student with a sense of historical and critical thinking, thus furnishing a more critical approach to medicine. It is not possible to measure the values which lie in a broadened understanding of man in his environment, an understanding which the physician must call upon every day in his work. The late Dr. Henry Sigerist drew the following comparison between the historian and the psychiatrist:

> The psychiatrist by analyzing a patient, often endeavors to make him aware

[4] *Ibid.*, p. 573.

of unconscious patterns of behavior so that he may face them openly and thus get rid of them. In the same way the historian, by analyzing historical developments, often tries to reveal and clarify unconscious social trends, so that by facing and discussing them openly we may improve conditions through intelligent action.[5]

The Role of the National Institutes of Health

Support in the history, philosophy, and cultural aspects of medicine is not a new area in the extramural programs of the National Institutes of Health. Several Institutes have supported compilations of historical information on the use of drugs and chemical agents, and historical surveys of particular diseases. The National Institute of Mental Health awarded a research grant for a critical historical review of manic-depressive psychosis, and, most recently, for a study on the cultural and social history of psychiatry. This year, the Institute awarded a training grant for an interdisciplinary program of research training in basic sciences relating to mental health, including provision for a survey of the historical developments of the biomedical sciences and of their relationship to other aspects of our culture, as well as funds for the university's department of the history of medicine.

Current programs of the Institutes which might be employed to assist development in the history, philosophy, and cultural aspects of medicine are: research grants (to support individual investigators, to provide funds for research conferences, to assist publication costs); undergraduate and graduate training grants to individuals and to university departments; and predoctoral and postdoctoral fellowship programs.

In view of the fact that the administration of the Institute has not yet transmitted the Council's earlier resolution on this subject to the other National Institutes of Health, having wished to explore the area, returning then to the Council for a general review, the Council might now wish to consider a broader resolution reasserting its support of N. I. H. program development in this area, recommending to the other National Advisory Councils and to the administration of the National Institutes of Health that they join with the National Institute of Mental Health in encouraging support for teaching and training activities, research and research tools in the history, philosophy, and cultural aspects of medicine; and recommending to the administration the setting up of an appropriate review panel to explore further the needs of the field as well as to review applications in this area.

Jeanne L. Brand, Ph. D.
Medical Research Program Specialist
Research Grants and Fellowships Branch
National Institute of Mental Health

---

[5] Henry Sigerist, M. D., *Medicine and Human Welfare* (New Haven: Yale University Press, 1941), pp. viii-ix.

NOTES AND COMMENTS

POSTSCRIPTUM ON JOHN MORGAN AND GIOVAN BATTISTA MORGAGNI [1]

ANTONIO PACE

Letters of John Morgan to his distinguished Scotch friend and colleague in medicine, Sir Alexander Dick, preserved in the latter's family mansion of Prestonfield near Edinburgh,[2] emerge to illuminate appreciably the trivial, perhaps, but piquant problem of whether the eminent Paduan physician Giovan Battista Morgagni did really inscribe a work to the young American doctor at the time of his visit to Italy in the summer of 1764, making playful allusion to the curious similarity of their names.[3]

For a century or more no shadow of doubt fell upon the words pronounced by Benjamin Rush in 1789 in his eulogistic *Account of the Late Dr. John Morgan, Delivered Before the Trustees and Students of Medicine in the College of Philadelphia* to the effect that Morgagni, " the light and ornament of two or three successive generations of physicians, was so pleased with the doctor, that he claimed kindred with him, from the resemblance of their names, and on the blank leaf of a copy of his works, which he presented to him, he inscribed with his own hand the following words, '*Affini suo, medico praeclarissimo, Johanni Morgan, donat auctor.*' " But brows were furrowed when, in more recent times, the inscription cited by Rush could not be found in Morgan's books in the

[1] This note is a by-product of research done in the summer of 1958 under a grant-in-aid from the American Philosophical Society.

John Morgan (1735-1789), trained at the University of Edinburgh, is known to students of American medical history as the founder of the first medical school in America (University of Pennsylvania), and Director-General of Hospitals and Physician-in-Chief of the Revolutionary army. Giovan Battista Morgagni (1682-1771), professor at the University of Padua, upheld gloriously the tradition of that ancient school in the field of medicine by laying the foundations for the study of pathological anatomy with his great work *De Sedibus et Causis Morborum* (1761).

[2] Microfilms in the Library of the American Philosophical Society.

[3] See: George Dock, " The Alleged Dedication of Morgagni Recognizing Kinship to John Morgan," *Medical Library and Historical Journal*, 1906, *4*: 41-44; Arturo Castiglioni, " Il dottor John Morgan da Filadelfia e Giovan Battista Morgagni," *Rassegna clinico-scientifica*, 1934, *12*: 385-391; Antonio Pace, " Notes on Dr. John Morgan and His Relations with Italian Men and Women of Science," *Bull. Hist. Med.*, 1945, *18*: 445-453.

library of the Philadelphia College of Physicians. Dr. George Dock, who first broached the riddle systematically in 1906, could bring before his public only the inscriptions in each of the two volumes of Morgagni's monumental *De Sedibus et Causis Morborum* once owned by Morgan—*Viro Experientissimo, et Humanissimo, D. D^i. Joanni Morgan auctor* in the first, and *Viro de Re Anatomica bene merito D^o. D^ni. Joanni Morgan auctor* in the second. Undaunted, Dr. Dock gave his reasons for believing that Rush had not suffered an hallucination: Rush's medical contemporaries, especially those associated with the College of Physicans, would not have ignored an error on such a conspicuous point; the absence of the original fly leaves in Morgan's copy of the *De Sedibus et Causis Morborum* (according to Rush the moot inscription was on a "blank leaf") admits an affirmative position; Morgan's journal of his continental tour, which mentions the gift of the *De Sedibus et Causis Morborum* but no inscription whatever, is obviously an incomplete account of the episode. Nearly forty years after Dock's study, I was able to widen the margin of probability that Rush was right by making two further observations: in addition to the *De Sedibus et Causis Morborum*, Morgagni appears to have given Morgan other books which might have been inscribed (Rush does not say that the inscription he cited was taken from the *De Sedibus et Causis Morborum*: had it been, the same breath would almost certainly have included the title of Morgagni's universally known work on pathological anatomy); there is evidence that Morgan's copy of Morgagni's *Epistolae Anatomicae Duae* (1762), at least, bore handwriting on its opening leaves before the volume was stripped, trimmed, and bound together with the similarly maltreated *Opuscula Miscellanea* (1762) by the famous Paduan professor.

Although Morgan's correspondence with Sir Alexander Dick does not clear up all the uncertainties, the shadows lift perceptibly. The letter written from Paris on 20 February 1764, a few days before Morgan left for Italy, arouses happy anticipation in the reader: "If I meet with anything curious in my rout, I shall be proud of laying it before you." In his next message, from Rome, on 16 June 1764, Morgan told Sir Alexander that his sojourn in Paris had enabled him to become acquainted with "some of the most celebrated Physicians in France & Italy," and that "I am also from hence to have a letter to my namesake the celebrated Morgagni at Padua." Morgan's journal reveals that he eventually obtained access to Morgagni, not with documents from Paris, but with a letter furnished him on 21 July by the Bolognese physician, Giuseppe Veratti. It is nevertheless interesting to observe at this point that Morgan

was aware of the resemblance of his name to Morgagni's before meeting the great Italian scientist. In fact, the abrupt manner in which Morgan brings up Morgagni as his "namesake" in writing to Sir Alexander Dick suggests strongly that the circumstance had already been the subject of amused commentary at Edinburgh.

Morgan's last, and most enlightening, report to Sir Alexander was sent from London on 10 November 1764, after his return from the tour:

> My Introduction to Morgagni at Padua was followed by the most familiar access to him during the whole time of my stay. He treated me with a profusion of Civilities; from the similarity of Name called me his Couzin, made me a present of his late grand publication de Sede et Causis Morborum, & with his own hand wrote in them what must have been flattering to one of distinction in the Profession, & spoke almost in raptures of a piece of anatomical preparation w.ch I gave him—made by myself.

Although not yet at the summit, we are now at a new plateau of understanding. Both Morgan's journal and his letter of 10 November, 1764, to Sir Alexander Dick make specific mention only of Morgagni's *De Sedibus et Causis Morborum,* presented on the occasion of the first encounter. The formal inscriptions in the two volumes of this work were surely calculated by the author to be "flattering to one of distinction in the Profession," and reflect Morgagni's sense of ceremony upon giving a copy of his masterpiece to his unexpected American caller. Under such circumstances the whimsical encomium cited by Rush would have been not only indecorously familiar, but superfluous, in view of the existing inscriptions. There can be no question, however, that the subject of the similarity of personal names did come up in the conversations between Morgan and Morgagni. A friendly play on the strange coincidence of forms, as Morgagni presented, perhaps on some subsequent and more intimate occasion, one of his minor works, would account for the inscription quoted by Rush in his eulogy of Morgan, which must have perished later in the process of binding.[4]

[4] The correspondence between Morgan and Sir Alexander Dick also documents another obscure event in the former's Italian travels, namely, his election to membership in the quaint Roman Accademia degli Arcadi, a fact heretofore known only from the title page of Morgan's historic *Discourse upon the Institution of Medical Schools in America* (Philadelphia, 1765), where the author identifies himself as a "Member of the Arcadian *Belles Lettres* Society at ROME." "Indeed," Morgan remarked in his letter from Rome, 16 June 1764, "the Nobility & english Gentlemen in Italy have treated me with great politeness, so that by their means I have been presented to the Duke of York, as well as to the Pope & several Italian princes & princesses, . . . & have been made a member of the arcadian society at Rome, of w.ch the Duke of York condescended to be made a member very lately."

MEDICO-HISTORICAL NEWS AND ACTIVITIES

CORRESPONDENCE AND REPORTS

SIXTEENTH INTERNATIONAL CONGRESS OF THE HISTORY OF MEDICINE

The Sixteenth International Congress of the History of Medicine took place at Montpellier, France, September 22 to 28, 1958, under the patronage of the President of the French Republic and the presidency of Dean Giraud of the Faculty of Medicine of Montpellier. About 125 persons from 22 nations were registered as members of the Congress, many of them accompanied by their wives. Members from the United States were Charles Cook (U. S. Embassy, Paris), George W. Corner (New York), Mae M. Linke (Washington, D. C.), Genevieve Miller (Cleveland), Robert J. Moes (Los Angeles), Thomas J. Moran (Pittsburgh), Charles D. O'Malley (Palo Alto), Vilma Proctor (Los Angeles), Theodore Rothman (Beverly Hills), Morris H. Saffron (Passaic), Rudolph E. Siegel (New York), and Allen C. Wooden (Wilmington, Del.). Four of these (Miller, Rothman, Siegel, Corner) presented papers.

Participants were impressed by the opportunity to meet in the Anatomical Theater with its ancient symbolic *cathedra*, a Roman ceremonial chair of marble brought centuries ago from the amphitheater of Nimes; and in the portrait-lined Salle des Actes before its shrine of Hippocrates inscribed (with justifiable pride on the part of the world's oldest active medical faculty) OLIM COUS NUNC MONSPELIENSIS HIPPOCRATES.

The program of the Congress was devoted to four themes, namely Medical Iconography in the 17th Century, Relations of the Montpellier School with other Medical Centers, the History of Hospitals, and Contributions of the New World to Therapeutics. Numerous papers on all these subjects were presented, as well as others classed as *Varia*. Because there were two separate sessions in progress at all times, it was not possible to hear all the papers and to report the communications in proper detail. Among those which seemed to awake special interest were one by J. Leibowitz of Jerusalem calling attention to a special contribution of mediaeval Jewish physicians to the development of medical education at Montpellier; a description by Mlle. M. L. David of Vincennes of certain engraved "images" or posters of the Paris confraternity of surgeons illustrating the traditions and equipment of its members in the 17th century, and a paper on pathology in Maya art by C. Martínez Duran of Guatemala. A talk on Montaigne as medical humanist and psychiatrist by Theodore Rothman of Los Angeles was warmly received by French colleagues.

Although attendance from Soviet-controlled Europe was limited, a large delegation expected from Rumania for example being conspicuously absent, a group of five Russian medical historians led by Academician Basil Ternovsky presented

papers and displayed an impressively large collection of recently published books on medical history. They obviously shared the desire of the U. S. delegation for improved acquaintance. It is a pleasure to report that not only in the meeting halls but also in social gatherings the two groups joined in friendly converse as far as permitted by the necessity of talking in French, and concluded one of their informal gatherings by raising their glasses together, in a fervid toast to peace and goodwill.

The Library of the Medical Faculty arranged a special exhibit of rare books, diplomas, and other memorabilia from its rich collection, among which the most striking item was the well-known 14th century manuscript of Guy de Chauliac's Surgery, opened to show its famous illustration of a human dissection. Colored reproductions of this picture and a medal commemorating the Congress were distributed to members. Space does not permit detailing the numerous entertainments and diversions provided by the hospitable hosts. These included visits to points of interest in Montpellier and vicinity, including the city's old mansions and the walled town of Aigues-Mortes. A tour of the ancient botanical garden was guided by its archivist, M. Herlant. Three civic organizations held receptions ("vins d'honneur") at which the noble Muscatel and other famous dessert wines of Languedoc were generously dispensed. On the second evening of the meeting a remarkable dramatic performance was given in the courtyard of the Medical Faculty, formerly the cathedral cloister, with the cathedral itself as background. This was an adaptation of Goethe's *Faust*, newly rendered into French by a Montpellier professor, and poignantly acted by a company including three former members of the Comédie Française. On another evening there was an outdoor concert of chamber music at a château in the country near Montpellier, given by the Viscount and Viscountess de Saporta in remembrance of their ancestors, Masters of the Montpellier Faculty of Medicine in the 15th, 16th, and 17th centuries.

At the closing banquet, at which the diners were entertained by a troupe of young people giving folk-dances of Languedoc and Provence, representatives of the various national societies took occasion to state, in their own languages and in French of every degree of competence, the gratitude of all to Professor Turchini, chairman, Dr. Dulieu, secretary-general, and others of the local committee, for the hospitality and practical arrangements of the Congress.

The Permanent Executive Committee of the International Association of Medical History met during the Congress and received tentative invitations to hold the next Congress at Athens or Edinburgh.

George W. Corner
Delegate of the American
Association for the History of Medicine

HISTORICAL MEDICAL MUSEUM IN TEL AVIV

It is always pleasant to announce an event in medical history, however limited in scope it may be. The late Dr. Sigerist held museums in this field in high esteem, and he devoted Appendix III to this item in his "A History of Medicine" (vol. I, pp. 525-531). Not every trained historian has sufficient inclination to active work in this domain. On the contrary, the modern historian is somehow reluctant to indulge in the 'antiquarianism" which seems to be a part of the museum sphere. Fortunately, the profession does not lack true collectors who are often the founders and competent keepers of medico-historical museums.

In November, 1957, a new museum devoted to medical history was established in Tel Aviv, Israel. It is located on the premises of the local branch of the Hebrew Medical Association, more specifically at its library. The holdings comprise: old diplomas of Jewish physicians; medical medals, bequeathed in part by the late Dr. Abraham Levinson of Chicago, pediatrician, historian, and collector; medical manuscripts; pictures of physicians; medical stamps and ex-libris; folklore. A nucleus of an historical medical library is attached to the collection.

Most of the exhibits were given from his private collection by Dr. S. J. Plaschkes, the founder of the museum and its Curator, whose competency in this field is much appreciated in this country. During the first year the holdings have been increased by gifts and loans from local and foreign friends, especially from Switzerland and Austria. There exists also in the U. S. A. a committee to support the young institution. Dr. Plaschkes, whose residence is 21 Sirkin Street, Tel Aviv, would be glad to hear from persons and institutions who may show interest in the museum.

JOSHUA O. LEIBOWITZ

BERTHA SPECTOR

It is with profound regret that the *Bulletin* records the death, on October 2, 1958, of Mrs. Benjamin Spector. Bertha Spector, known affectionately to innumerable friends as "Bert," was trained as a lawyer and was active in her own profession until the time of her death. She was a familiar figure at all meetings of the American Association for the History of Medicine, as well as a participant with Dr. Spector in the graduate weeks which have been held from time to time at the Johns Hopkins Institute of the History of Medicine. Her interest in the field was real and of the kind which works behind the scenes through genuine concern for the people involved and time-consuming labor at the side of her husband during his terms of office as Treasurer, Secretary, and President of the Association. It is clearly impossible to assess the debt of the Association to Bertha Spector, for her work for it was always informal in character and would, indeed, have been anonymous, were it not for the fact that it naturally centered around the historical and organizational work of the husband to whom she was devoted. The *Bulletin* takes this opportunity to extend deep sympathy to Dr. Benjamin Spector and to assure him that many friends will miss "Bert" at the next meeting of the Association.

OWSEI TEMKIN AND C. LILIAN TEMKIN

ANNOUNCEMENTS

AMERICAN ASSOCIATION FOR THE HISTORY OF MEDICINE

Thirty-Second Annual Meeting: Cleveland, May 21-23, 1959

The 32nd Annual Meeting of the American Association for the History of Medicine will take place in Cleveland, Ohio, May 21 to 23, 1959, at the Cleveland Medical Library, 11000 Euclid Avenue. The Wade Park Manor, East 107th & Chester Avenue, will be the headquarters hotel. The tentative program foresees two general sessions and two symposia. Dr. W. B. McDaniel 2d will give the Garrison Lecture. Special exhibits are foreseen at the Cleveland Health Museum. Baldwin Wallace College, Berea, Ohio, has invited the members of the Association to be guests at the performance of the B Minor Mass on Saturday, May 23, during its Twenty-Seventh Annual Bach Festival.

Chairman of the Program Committee: Miss Elizabeth H. Thomson (New Haven); Chairman of the Local Arrangements Committee: Bruno Gebhard, M. D. (Cleveland).

NATIONAL NEWS

College of Physicians of Philadelphia, Section on Medical History

The second Edward Bell Krumbhaar Award was presented to Dr. Donald E. Willard, Jr., 1958 graduate of Jefferson Medical College now surgical intern at The New York Hospital, on October 21, 1958. His essay, "W. W. Keen's 'First Modern Brain Case,'" was judged the best of six entries for 1958 by a committee consisting of Drs. Arthur M. Dannenberg, Herbert J. Dietrich, Jr., and Samuel X. Radbill. The Award of one hundred dollars and a prize certificate was presented by Dr. Krumbhaar to Dr. and Mrs. Willard, on behalf of the Section on Medical History. (The prize essay will be published in the *Transactions & Studies of the College of Physicians of Philadelphia* for February, 1959). This paper was read by Dr. Willard at the College Hall on March 11, 1958, at a meeting co-sponsored by the Osler Medical History Club of Jefferson Medical College. Dr. David J. LaFia is faculty advisor to the Jefferson group.

The Section's annual medical history essay contest, open to all students in Philadelphia's five medical schools, is now inviting entries for its third Edward Bell Krumbhaar Award, to be made late in 1959.

Herbert J. Dietrich, Jr., *Clerk*

Dexter Award in the History of Chemistry

The Division of History of Chemistry of the American Chemical Society is asking for nominees to be considered for the 1959 award which consists of a suitable plaque and $500. All information, in duplicate, should be sent to the Secretary of the Division of History of Chemistry, Mr. Sidney M. Edelstein, Dexter Chemical Corporation, 819-45 Edgewater Road, Bronx 59, N. Y., by March 10, 1959.

Johns Hopkins University

Kenneth D. Keele, M. D., F. R. C. P., Visiting Professor of the History of Medicine at Yale University, gave a Hideyo Noguchi Lecture at the Johns Hopkins Institute of the History of Medicine on December 12, 1958. Dr. Keele spoke on "Leonardo da Vinci: The Man and his Work."

New York Academy of Medicine

The 1959 Eastern States Health Education Conference will be held on April 23 and 24, 1959, at The New York Academy of Medicine, 2 East 103 Street, New York City.

University of Illinois

The 1959 D. J. Davis Memorial Lecture on Medical History will be given at 1:00 p. m., April 22, 1959, in Room 221 of the Dental-Medical-Pharmacy Building, 1853 W. Polk Street, Chicago 12, Illinois. The lecturer will be Dr. Lloyd G. Stevenson who will speak on "The Early Development of Internal Medicine in the United States."

PROGRAMS OF MEETINGS

Beaumont Medical Club (Yale)

October 10, 1958: "Prediction of Sex: Folklore and Science," Thomas R. Forbes.

December 9, 1958: Beaumont Lecture: "The Genesis of Mona Lisa," Kenneth D. Keele.

Benjamin Waterhouse Medical History Society (Boston)

January 5, 1959: "Inoculation Controversy of 1721," Donald Fleming.

February 9, 1959: "Podagra—History and Martyrs," George Thornton and Arnold Wong.

College of Physicians of Philadelphia, Section on Medical History

February 17, 1959: "A Mock Epic Poem: 'La Guerre des Venetes,' by Laennec," William P. Boger; "The Story of Herbs in Medicine," Douglas Macfarlan; "Dr. Robley Dunglison and Jefferson," Samuel X. Radbill; "Mark Akenside: Eighteenth Century Physician-Poet," Herbert J. Dietrich, Jr.

April 7, 1959: Annual Kate Hurd Mead Lecture: "Wit in Rabelais, Sterne and Osler: Kindred Spirits," Herbert F. West.

Society of Medical History of Chicago

December 17, 1958: "The Vienna School of Pediatrics and the Concept of Allergy," Helmut P. G. Seckel, and "Ladies of Lynn—Emphasis on One," E. Lee Strohl.

January 14, 1959: "Francesco Redi: Physician, Microbiologist and Poet," Eric W. Cochrane, and "Surgical Pathology in the Renaissance," Leo M. Zimmerman.

February 11, 1959: "Hermann Boerhaave and the Rise of Clinical Teaching," Steven Armentrout, and "Antyllus, Surgical Genius of Antiquity," Richard L. Grant.

The Johns Hopkins Medical History Club

February 24, 1959: "Vesaliana (A Demonstration), William L. Straus, Jr., and "William S. Halsted's First Paper (Carbon Monoxide Poisoning, 1883)," Saul Jarcho.

NEWS FROM ABROAD

Exhibition of Medical Records

The *Bulletin* has received the catalogue of an exhibition held at the Royal College of Physicians, London, in connection with the British Records Association Conference, November 25 and 26, 1958. The period concerned coincides roughly with the foundation of the Royal College of Physicians (1518) whose charter is included in the 63 items, down to the Medical Act of 1858. The items are grouped under four main headings: The Medical Profession, Medical Education, Medical Practice, and the Care of the Sick. The catalogue was edited by L. M. Payne, F. L. A., Assistant Librarian of the Royal College of Physicians, who also wrote the historical introduction.

The Bibliotheck

Under this name a new "journal of bibliographical notes and queries mainly of Scottish interest" has been launched, edited by L. Jolley. The *Bulletin* has received the third number of the first volume and draws particular attention to the bibliography of the printed works of William Hunter, by W. R. Le Fanu, and to "A note on the portraiture of William Cullen," by L. Jolley, which contains photographs of a David Allen drawing of Cullen and of Cullen's death mask in the Hunterian Museum, Glasgow.

The Norwegian Society for the History of Medicine

The new journal, *Medical History*, published in London, is also the official organ of the Norwegian Society for the History of Medicine. The Society, founded in 1956 with headquarters in Oslo, has a growing library. Officers of the Society are: Professor Axel Ström, president; Prosector Dr. med. Bernhard Getz, Anatomic Institute, University of Oslo, secretary.

LYNN THORNDIKE. *A History of Magic and Experimental Science.* Volume VII and VIII, The Seventeenth Century. New York: Columbia University Press, 1958. Vol. VII: x + 695 pp. Vol. VIII: viii + 808 pp. $10.00 each.

The two volumes under notice bring Thorndike's monumental work to a close. Its first volume which appeared in 1923 started a new epoch in scientific and medical historiography by placing at the disposal of the scholar a work of reference and new sources with no equal in width and depth of scholarship. What is no less important, it has its own story and moral: the interlocking of scientific achievement with what is briefly called: Magic. Never before had this been demonstrated on such a broad basis. In the course of the centuries, however, the meaning of Magic has undergone a change. This change is quantitative as well as qualitative, since the actual bulk of magic was diminishing fast when the XVIth and XVIIth centuries were reached. Moreover, none of the Magic of the Picatrix or the Magic Papyri could be associated with Francis Bacon, with Mersenne and Gassendi, with Harvey, Descartes, Huygens, Boyle, Kepler, Galileo, or Newton. All the same, these founders and luminaries of modern science and medicine are among the *dramatis personae* of the volumes under notice. We are told that Descartes' " contemporaries thought of him as a magician who controlled Nature's secrets." The Cartesian attitude of doubt and his mechanistic explanation of the universe were not directed against belief in the influence of the stars and in " marvels." " Descartes was so confident in his ability to explain anything and everything by his principles, that he was apt to employ them in justifying such dubious beliefs as that in inexhaustible lamps, or in the bleeding of the corpse of the victim in the presence of the murderer, as well as in expounding genuine natural phenomena. He had no doubt that he could discover the art of prolonging human life " (VII, 557). This attitude does not seem to differ from that of such mediaeval " magicians " as Roger Bacon or Paracelsus and Van Helmont in the XVIth and XVIIth centuries. For it was these who refused to deny the existence of " magic " phenomena, being intent on explaining such phenomena in terms of physics and chemistry—in contrast to their enlightened contemporaries, who either declared them fictitious or called in the devil or other supernatural forces to account for them. A classical example, treated at length by Thorndike, is the " weapon salve " reputed to act on the wound when applied to the sword which had caused it, even at great distances. As Halley said, the third book of Newton's *Principia* was a revelation of the " abstrusest secrets of nature," and in Lord Keynes ' opinion Newton was " a magician because the discovery of secrets of nature was his aim." He was clearly " an unbridled addict " to alchemy and many of his unedited manuscripts are " wholly magical and wholly devoid of scientific value "—although he devoted years of work to them during the twenty-five years of his mathematical studies. " His deepest instincts were occult, esoteric, semantic, with profound shrinking from the world." Kepler's leanings towards occult and animistic interpretations and his belief in astrology are well known. " Galileo was not so addicted to astrology as Kepler, but he had read . . .

astrological writings . . . and also composed many genitures," and not until 1633 would he seem to have lost faith in astrology (VII, 36). All this illustrates the changes in the meaning of "Magic": in the period treated, it comprises any non-scientific beliefs and motives that are found in a given scientist or associated with the treatment of scientific subjects and the achievement of genuine scientific results. This kind of "magic" gradually wore down with the progress of science and was reduced to a "residue" in the century that saw the foundation of modern science and forms the subject of the book. Yet, as Thorndike emphasizes, there is no reason for complacency: the residue was substantial and affected the best minds of the age. Nor should we forget that in spite of a different "ratio" of magic to science, the Middle Ages had anticipated much of XVIIth century mechanics and such scientific movements as the opposition to the ancient doctrine of the elements. "Most modern physicists do not repeat Galileo's mistake of studying only ancient science and neglecting mediaeval activity in physics, for the simple reason that they do not pay any attention to the history of science. But is not this doubling his error?" (VII, 44). We may add that any amount of "magic residue" may come to the surface again at any moment when science is used for political propaganda—it was the magic of racialism that induced "scientific" geneticists and anthropologists to lend support to the massacres and to the complete perversion of science and the medical art which we have witnessed in our own time.

No detailed account of the contents of Thorndike's last two volumes can be attempted here. We have chapters in which the scientific work of the "great" and their sources are elucidated and much new information is collected. It is pointed out that Sennert really took a small part, if any, in the revival of the atomic (corpuscular) theory—in contrast to Beeckman who applied it as early as 1613-14. In the chapter on Mersenne and Gassendi (including an account of Fludd), the association of scepticism and credulity stands out as a feature typical of the time. In the following up of Harvey's discovery, Beeckman is given as the first to recognize it. "This was quick work on Beeckman's part (1633), since he read Gilbert on the magnet for the first time only in 1627, and did not see Kepler on the movement of Mars until 1628" (VII, 518). Those interested in the "magical" ("microcosmic" and astrological) interpretation of Harvey's discovery (Thorndike mentions Claude Tardy, 1657, in this connection) should note that the first recognition actually came from the mystic and Rosicrucian, Robert Fludd, 1629-31 (*see* this *Bulletin*, 1935, *3*: 277-8). Perhaps the "Hippocratic revivalist," Raymond Restaurand, belongs to this category of "magical" interpreters of Harvey and deserves quotation. The list could be enlarged (*see J. Hist. Med. & Allied Sc.*, 1957, *12*: 140-57). The chapter on Harvey is headed: "Harvey and Patin," who may at first sight impress the reader as strange bedfellows. However, it was Patin who led the controversy about blood letting at Harvey's time, and its discussion logically follows that of transfusion and the influence that Harvey's discovery had on its development. Moreover, this joining of Harvey, the experimenter and discoverer, with Patin, the dogmatic conservative, teaches the lesson that "the conservative is the less animistic, astrological and magical of the two, while the progressive is the more so. The scoffer at superstition has his own cherished delusion. The discoverer of hitherto concealed truth still adheres in many respects to Aristotle and shares many of the mistaken notions of his time. Patin could detect folly; Harvey had faith in Nature. Patin's future influence on medicine is

negligible . . ." (VII, 543). Van Helmont's discovery of "Gas" and gases is given no more than an incidental mention, chiefly as a progressive enrichment of terminology, although it owes much to neo-Platonic pneumatology and mysticism. To Van Helmont the natural object in its gaseous form, i. e., divested of its material cover through combustion, has retained all that is essential or specific for that particular object—the object *is* its "gas," and the latter the material vector of its specificity. In this Van Helmont believed that he had given a test-tube demonstration of the "spiritual" ("astral") bodies, or the *logos* and soul that is intrinsic in all objects of creation. A similar example is Van Helmont's belief in water as the only "element" from which all objects derive—a belief which is largely based on gnostic-alchemical and biblical tradition. Thorndike's chapter on Van Helmont is of value—not only for a fairly full catalogue of his beliefs (this incidentally includes, VII, 229, the *Alphabeti vere naturalis Hebraici . . . delineatio*, a product of his son Francis Mercury: Sulzbach, 1657), but also for an account of his great influence which has often been underrated. The interesting figure of Marcus Marci of Kronland is briefly discussed and his *Idearum operatricium idea* found not "to lead to as much magic as might be expected" (VII, 313). Yet his anticipation of *Feldtheorie* in embryology apppears as the strange product of a preoccupation with the metaphysics of light and with Aristotelian and Paracelsian ideas, combined with sound observation and reasoning (*see* this *Bulletin*, 1935, *3*: 224-231, and Joseph Needham, *History of Embryology*, Cambridge, 1934, p. 62). From recent research (V. Kruta in *Physiologia Bohemoslovenica*, 1957, *6*: 433) it would appear highly probable that Harvey met Marci at Prague in 1636. Marci's views on epigenesis were certainly congenial to those of Harvey and may in fact have influenced him (*see Isis*, 1951, *42*: 32).

These few examples of a direct influence of "magic," i. e., non-scientific, ideas on scientific progress may suffice. It will be the task of the future to increase and elaborate such instances in detail—and it is for this that the stupendous collection and first sifting of material given by Thorndike will prove the indispensable basis. There does not seem anything that has escaped his Herculean labour in the representation of astrology before and after 1650, in alchemy and iatrochemistry, in mineralogy, in natural magic (including Portugal and Spain), in "occult" work at German universities, in the *cursus philosophicus* and *physicus* before Descartes, the Scotist revival, the "Underground World" of Kircher and Becher, artificial magic and technology, in natural history, the work of the Academies, miscellaneous and popular science, chemical courses and manuals after 1650, in medicine and physiology, physiognomy and divination, in mental disease and magic. Against the many hundreds of names and accounts in these two volumes it will be difficult to think of any further names that might qualify for inclusion. Some minor figures who might be considered in this connection are: Samuel Hafenreffer, the "Rosicrucian" interpreter of the pulse on Fludd's principles and the first to classify skin disease; Rich. Vitus of Basingstoke and his epistolary enquiry into the Aelia Laelia Crispis—riddle; and Jac. Tollius, the explorer of alchemy in terms of Greek mythology.

The reviewer feels bound to express the deep gratitude that must inspire any user of this work for an unparalleled wealth of information, for the critical lead into the tangle of highly obscure and equivocal ideas and doctrines, and the new historical understanding of the origins and meaning of modern science which Thorndike's studies will make possible.

WALTER PAGEL

FRANS JONCKHEERE: *Les médecins de l'Egypte pharaonique*, Essai de Prosographie. (*La médecine égyptienne*, No. 3). Bruxelles: Édition de la Fondation Égyptologique Reine Élisabeth, 1958. 172 pp. Ill.

The work under view is limited by the author to a study of the individuals, exclusively of the pharaonic period (i. e. Dynasties I-XXX), identified by the title *sinw*, "doctor, physician," or by the designation *irỉ ibḥ* or *ibḥy*, "dentist." The nature of the study precludes a claim to completeness on the part of the author, and it is further regretful that he was unable to bring several of the chapters to a finished state before his death. The valuable service of editing the work for publication was done by the noted Egyptologist, B. van de Walle, to whom we are doubtlessly indebted for assuring the general scholarly excellence of the finished publication.

The first chapter constitutes the major portion of the work and consists of an alphabetical (Egyptian) list of all persons mentioned throughout the vast corpus of literary and epigraphic remains from ancient Egypt who qualify for inclusion on the basis of the above stated criterion. Each name is accompanied by a careful Egyptological transcription, a "popular" transcription, a full description of the particular monuments on which the individual is named, and a complete bibliography of pertinent material. The reviewer is particularly impressed by the completeness of the bibliographical references, which will remain an outstanding contribution of the essay. The second chapter provides the same names in chronological sequence.

Chapters III and IV treat a variety of special problems which arise in connection with the titles and personal names already listed. The author's statements are cautious throughout and reflect a desire to present information rather than to engage in possible controversies over its interpretation. Because the understanding of the physician's role in ancient Egypt demands that every bit of evidence be utilized, Chapter V contains a detailed account of the non-medical titles associated with the persons named and comments on the significance of the various combinations. Among the more common of these related disciplines are those of religious and magical content, such as the *ḫry-ḥb*, "lector priest," the *ḫrp srq.t*, "one empowered by the goddess Selkis," and the *ḥm-nṯr Ḥk3*, "priest of Heka (Magic)," as well as theordinary title for priest, *w'b*. The reviewer is still disinclined to accept the identification of Egyptian *ḫry-tp* with Hebrew *ḥarṭummîm*, as suggested on p. 126; cf. my remarks in *JAOS* 73 (1953), 150-151.

After a short section devoted to the mention of the *sinw* in medical, religious, and literary texts, the author deals with the orthography of the word itself in hieroglyphic, a valuable adjunct to the still infant subject of Egyptian lexicography. As the editor points out (p. 151, n. 1), M. Jonckheere cites, but does not follow, the *plene* writing of the word *swnw* instead of *sinw*. The reviewer would note that the reading *swnw* is supported also by other indirect *plene* writings of words using the "arrow" hieroglyph in one of their variant spellings. It is curious, by the way, that *WB* III, 427, does not supply a reference for this particularly important and unique *plene* spelling (presumably from the Middle Kingdom). The Coptic survival of *swnw* is *saein*, etc., which suggests a development **sāwin* > **sāyin* > Proto-Coptic *sêyn* for the stem of the word.

The study closes with a short chapter, apparently unfinished, devoted to the social

status of the physician in ancient Egypt and an iconographic index to the excellent plates at the end of the volume. Well conceived, carefully executed, and beautifully printed with a minimum of typographical errors, M. Jonckheere's essay constitutes a valuable addition to the study of ancient medicine and to Egyptian prosopography.

THOMAS O. LAMBDIN

GEORGE ROSEN. *A History of Public Health.* (MD Monographs on Medical History Number One). New York: MD Publications, Inc., 1958. 551 pp. $5.75.

It is an illuminating commentary on the attitudes and interests of medical historians of the past that only now do we have for the first time a study covering in its broad sweep the development of that branch of medicine which has had the greatest influence on people at large, namely, public health. While several excellent volumes have discussed various aspects of public health, or public health in certain times or places, Dr. Rosen has surveyed concepts of disease, scientific developments, disease conditions, and public health measures, including public provisions for medical care, from antiquity to the present, pointing out for successive ages the significance of underlying social, economic, and political beliefs and conditions. In so broad a survey, much of the material is inevitably familiar to those versed in the history of public health, and some topics necessarily receive rather scant attention. The focus is concentrated on western Europe (chiefly England, France, and Germany) and northeastern United States, and nearly two thirds of the book is devoted to the last 130 years. Nevertheless, Dr. Rosen has not only demonstrated his thorough knowledge of the available secondary material but has also dug deeply into the original sources of public health history, to produce a comprehensive, well-rounded synthesis that will be of interest and value not only to those who are concerned with medical and public health activities, but to all who are interested in the history of our civilization.

In addition to the text, the volume under review contains about 10 pages of bibliography, 9 pages of "Memorable Figures," and 10 pages listing current periodicals, societies, and schools of public health. In this reviewer's opinion, the last section might well have been omitted—this being the sort of information one seeks in frequently revised directories—and the space used to augment the bibliography. In a general survey of this nature one does not expect extensive citations of authority. The present bibliography, however, which consists of an unannotated listing of books and articles for each chapter, appears to be a not altogether successful compromise between a true bibliography and a list of references. It manifestly does not include all works consulted, or even all those mentioned, be it only by author, in the text. (On p. 69, for example, Dr. Rosen writes: "According to Clemow, . . ." but Clemow is nowhere listed in footnote or bibliography.) A brief critical bibliographical essay and guide to further reading would, I believe, have proved more useful and stimulating to the majority of Dr. Rosen's audience.

A History of Public Health is the first of a series of planned MD Monographs on Medical History. While I cannot agree with the publisher's enthusiastic blurb

that the volume exemplifies "the ultimate elegance in the art of bookmaking," the author has set a high standard for future contributors to the series. His book makes clear that public health has a proud record of achievement. It may justly serve as an inspiration to those who face the many problems, some of them caused by the very successes of the past, which still challenge health workers throughout the world today.

JOHN B. BLAKE

H. H. CUNNINGHAM. *Doctors in Gray*, The Confederate Medical Service. Baton Rouge: Louisiana State University Press, 1958. xii + 339 pp. Ill. $6.00.

Doctors in Gray is a well documented work and one that most assuredly will be a valuable contribution to the history of medicine. It should also be of value to all historians interested in the Civil War Period. In the opinion of this reviewer, the book suffers chiefly from the author's attempt to consolidate into one volume material that could have been handled more easily in a work of three or four volumes. Frequently this reader gained the impression that he was reading extracts from official records, reports, and statistical summaries.

The first chapter, entitled "The Confederate Medical Problem," is replete with such statistics, so that the reviewer experienced considerable difficulty in understanding the author's concept of the problem. Historians of the Civil War generally agree that paucity of qualified physicians with military experience, budgetary problems, and political interference were the major problems encountered by the surgeon general and his staff. Unfortunately these factors are not emphasized in the author's evaluation of the situation; this omission weakens the first part of the book. The reader's interest could have been captured with ease if the problem had been defined in narrative style, rather than by the use of statistics. Cunningham, by allowing these important aspects of the problem to become lost in a series of casualty reports, failed to achieve his objective.

On the other hand, the chapter covering the medical background of Confederate Doctors is excellent. This section is not only well written, but here information of value concerning the status of medical qualifications, education of the period, the origins of southern medical schools, professional societies, and scientific journals, is presented in a brief and concise manner. This section is not only entertaining but because of the manner in which the material is presented and the thoroughness of its documentation, it is also valuable for reference use. Here the lucidity and clarity of the author's style maintains the reader's interest.

The section dealing with southern medical education in the war period contains data that are of importance to the historian. The manner in which the author has reviewed the problem faced by the medical schools of the period, the curriculum of the day, the opinions of the students of the curriculum and the faculties, is excellent. The sections dealing with the closure of the medical schools during the war years and the effect of their closure on both the military and civilian population, make enjoyable reading.

The appraisal of Samuel Preston Moore, who succeeded the first surgeon general, David Delone, is fair and unbiased, and the chapters which review the

establishment and organization of the army's general hospitals and the problems encountered in their administration contain valuable data which are presented in a well organized manner. Certainly the documentation of this section reveals the pains-taking research work performed by the author. Again, in the chapter dealing with the prison hospitals, much new information is presented in an objective and unbiased manner. The description of the difficulties encountered in the procurement of medical supplies is also excellent. But, the author's treatment of the causes of disease, the prevalence of disease, and the surgical procedures used by the field surgeons, is not as readable as might be desired. Here again the reader becomes aware of the author's attempt to document his material by using data gleaned from hospital statistical reports. Perhaps the most entertaining sections of the book are the chapter devoted to the medical officers serving in the field and the appraisal of the Confederate Medical Officers. In these two sections the material is presented in a readable manner.

A review of *Doctors in Gray* would be incomplete if mention was not made of the magnificent bibliography divided into the following sections: 1 – Manuscripts 2 – Official Documents 3 – Collected Sources 4 – Diaries, Journals and Travel Accounts 5 – Miscellaneous Contemporary books and pamphlets 6 – Periodicals and Newspapers 7 – Autobiographies, Memoirs and Reminiscences 8 – Biographies 9 – Monographs and Special Studies 10 – Medical History 11 – General Works. This method of enumerating documented references will be valuable not only to the medical historian but for the general historian as well. It is a most carefully compiled and arranged bibliography, and one that serves to make the book a valuable historical work.

In summary, *Doctors in Gray* is a most authoritative work. If there is any justifiable criticism, it is perhaps the comment that the author's attempt to consolidate his material detracts from the book's readability.

E. M. Holmes, Jr.

[E. M. Holmes, Jr., Editor]. *The Selected Writings of Marvin Pierce Rucker and Complete Bibliography of His Works.* Privately Printed. Richmond: Whittet and Shepperson, 1958. xviii + 198 pp. Ill. [Offered for sale at $7.50 for the regular edition and $25.00 for the memorial edition by the Marvin Pierce Rucker Memorial Association. Orders for the book should be addressed to the Marvin Pierce Rucker Memorial Association in care of the Richmond Academy of Medicine, 1200 East Clay Street, Richmond 19, Virginia.]

Pierce Rucker, a Virginia physician who died in 1953, was for a period of ten years the editor of the *Virginia Medical Monthly.* To that publication he contributed a series of brief biographical sketches of physicians and these have now been published in book form as a tribute to his memory. The biographical notes of the Johns Hopkins group are frequently enlivened by the personal touch, Rucker calling on his memory for details of his student days and the impressions that his great teachers — Osler, Halsted, Kelly and Welch — made upon his active mind. But even of more importance are the vignettes of Virginians whose names are little

known to medical historians, such as John Peter Mettauer, whose father, a French army surgeon, came to America with LaFayette and Rochambeau and after the surrender at Yorktown settled in Virginia. The son became Virginia's greatest gynecologist. Joseph Lyon Miller, a physician who took a contract practice with a coal mining company after his graduation, remained in Thomas, West Virginia, the rest of his life. In this remote town Miller established himself not only as an excellent practitioner of medicine but as a book collector with extraordinary talents. He was well known to all the dealers in antiquarian medical literature in Europe and in this dusty mining town he assembled a library of unusual worth, now housed in the Richmond Academy of Medicine.

These "Pen Profiles," as Dr. Rucker called them, are well worth re-publishing but unfortunately they are not indexed and the medical historian will find this lack of an essential bibliographical tool a considerable handicap. If the reader decides to make his own index he will find that his time is well spent, for many of the facts imbedded in these sketches are not to be found elsewhere. This is particularly true in regard to the lives of the Virginia physicians, a group that Rucker knew so well.

The lack of an index is felt even more keenly in the second part of the book which deals with "Floral Eponyms." Dr. Rucker collected 118 examples of flowers or plants that carry the name of either a physician or someone closely associated with medicine. Many of the names are familiar to all, such as Garden, Wistar, Hosack, Magnol, Poinsett, Fuchs, and Withering, but others are less known in connection with floral eponyms, — Eschscholtz, Dahl, Zollikofer, Alpinus. This section is an important part of the book, for here are set forth, much of it for the first time, data of considerable value for the medical historian, and even a list of the doctors so honored would have been useful.

This beautifully printed book is a fitting memorial to a lovable physican who accomplished so much for his native state of Virginia. He served as a health officer and was one of the first obstetrical specialists to practice in Richmond. He was a much respected physician who won the confidence of his patients and the respect of his brother physicians. The book is preceded by an appreciative biography written by Rucker's close friend and colleague, Dr. J. Morrison Hutcheson.

HENRY R. VIETS

IAGO GALDSTON (editor). *The Impact of the Antibiotics on Medicine and Society.* Monograph II. Institute of Social and Historical Medicine, The New York Academy of Medicine. New York: International Universities Press, Inc., 1958. x + 222 pp. $5.00.

This book contains a series of lectures given at the second meeting of the Institute of Social and Historical Medicine. The Institute was organized for the purpose of applying medical history to the study of contemporary medical issues and problems, and in this particular case, to a socio-and medical-historical inquiry into the advent of the antibiotics. The lectures may be divided into three groups; historical, present-day use of antibiotics, and general description of present-day medicine. In the historical group are excellent descriptions of Galenicals by Owsei Temkin, Galenicals and Galenism in the History of Medicine; Lloyd G. Stevenson,

Antibacterial and Antibiotic Concepts in Early Bacteriological Studies and in Ehrlich's Chemotherapy; Lynn Thorndike, The Continued Prevalence of the Conception of Macrocosm and Microcosm in the Seventeenth Century. The present-day use of antibiotics was discussed by Selman A. Waksman, The Microbiology of the Soil and the Antibiotics; Henry Welch, Antibiotics 1943-1955: Their Development Role in Present-Day Society; John E. McKeen, The Role of Industry in the Mass Production of Antibiotics; C. C. Dauer, A Demographic Analysis of Recent Changes in Mortality, Morbidity, and Age Group Distribution in our Population; Herbert G. Luther, The Application of Antibiotics to the Livestock Industry; Louis G. Nickell, The application of Antibiotics to Plant Husbandry; Walter C. Alvarez, The Antibiotics and the Clinical Practice of Medicine; John F. Mahoney, The Effects of the Antibiotics on the Concepts and Practices of Public Health; Lawrence B. Hobson, The Effects of the Antibiotics on Society. General talks were given by Willard C. Rappleye, Medical Research and Education in Relation to Industry, and by Eli Ginzberg, The Hospital and the Community: Some Dynamic Interrelations. There is a Foreword by Alexander T. Martin and an Introduction by Iago Galdston.

The question may be raised as to whether or not the purpose of the institute was achieved by the publication of this book. Some of the reports were only superficial discussions of the subject. There are many omissions, for example, the changes were not discussed that have been made in surgery and pediatrics through the use of antibiotics. Only a few of the articles have references. The general discussion of each paper presented by members of the audience after each paper was not published in the book. A medical historian of the future writing on the impact of the antibiotics upon medicine and society will not find much material in this book.

CHALMERS L. GEMMILL

OTIS MARSHALL. *Memoirs of a G. P.* New York: Vantage Press, 1958. 155 pp. $3.50.

In this short volume of autobiography, Dr. Otis Marshall tells his life story in a most unassuming manner. The account is completely disarming and as I read it I thought that Dr. Marshall had probably edited his diary for benefit of publication. At any rate, here is the story of a Virginia doctor of fifty years' experience, looking back over those years and the preparatory years before them. It is an account of everwidening horizons from boyhood to active retirement. Years of sustained service as a general practitioner, military service and service with the American Red Cross at home and abroad—all pass in review.

Here is the general practitioner of our century, who has led a devoted life of medical practice with all its hardships and occasional rewards, surveying his era—now past—and finding it good. Awareness of rapid changes in twentieth century medicine and a strong effort to "keep up to date," are evident in the closing chapters. Life is good and still enjoyed to the full as the story closes.

I have never read an autobiography with less literary pretension and such relaxed style. It will surely be enjoyed by all who maintain a shelf for country doctor sagas, for the author's humanity and humility stand out on every page.

ROBERT J. FAULCONER

THE BULLETIN RECENTLY RECEIVED *

Annual Report of the Surgeon General United States Army, Fiscal Year 1958. Washington, D. C.: Office of the Surgeon General, Department of the Army. vii + 94 pp. Ill.

Ricardo Archila. *Historia Médica de Venezuela: Guayana.* Caracas: Imprenta Nacional, 1958. 174 pp. Ill.

Hilding Bergstrand. *Svenska Läkaresällskapet 150 AR. Dess Tillkomst och Utveckling.* Stockholm: 1958, 562 pp. Ill. 50 Sw. Crowns.

Catalogue of an Exhibition of Medical Records in the Library of the Royal College of Physicians of London. With an Introduction by L. M. Payne. London: British Records Association, 1958. 40 pp. (Obtainable from the Honorary Secretary, British Records Association, The Charterhouse, London, E. C. 1. 3s. postpaid.)

Galen. *De Pulsibus ad Tyrones.* Traduzione e commento di Roberto Trifogli. Roma: A cura dell'Istituto di Storia della Medicina dell'Università di Roma, 1958. 48 pp.

William Carleton Gibson. *Young Endeavour.* Contributions to Science by Medical Students of the Past Four Centuries. Springfield, Illinois: Charles C Thomas, 1958. xx + 292 pp. Ill. $6.50.

Thomas H. Grainger, Jr. *A Guide to the History of Bacteriology.* New York: The Ronald Press Company, 1958. xi + 210 pp. $4.50 (*Chronica Botanica*: An international biological and agricultural series. No. 18).

William S. Heckscher. *Rembrandt's Anatomy of Dr. Nicolaas Tulp.* An Iconological Study. Washington Square: New York University Press, 1958. x + 283 pp. Ill. $15.00.

Sir William Hume. "The Medical Societies of Newcastle upon Tyne." Reprinted from: *The Newcastle Medical Journal,* 25: 163-226, September 1958. Ill.

Stanislas Konopka. *Les relations culturelles entre la France et la Pologne dans le domaine de la médecine.* Varsovie (Poland): Société Polonaise d'Histoire de la Médecine, 1958. 22 pp. Ill.

Ralph H. Major. *Disease and Destiny; Logan Clendening.* (Logan Clendening Lectures on the History and Philosophy of Medicine. Eighth Series). Lawrence, Kansas: University of Kansas Press, 1958. 49 pp. Ill. $2.00.

Marcello Malpighi. *De Pulmonibus.* Ristampa, traduzione italiana e introduzione a cura di Luigi Belloni. Per la Fondazione della Società Italiana di Istochimica e i Congressi Nazionali di Anatomia e di Medicina Interna, Messina Palermo, Ottobre 1958. 72 pp. Ill.

Medical Department United States Army. *Preventive Medicine in World War II*: Volume IV, Communicable Diseases Transmitted Chiefly Through Respiratory and Alimentary Tracts. Edited by John Boyd Coates, Jr. and Ebbe

* The *Bulletin* reserves freedom of decision as to the publications to be listed in this section. Items received, other than those assigned for review, are ultimately incorporated into the collection of the Institute of the History of Medicine.

Curtis Hoff. Washington, D. C.: Office of the Surgeon General, Department of the Army, 1958. xxi + 544 pp. Ill. $5.50.

D. De Moulin. "Some Remarks on the Early History of Blood Transfusion." Reprinted from *Archivum Chirurgicum Neerlandicum*, 10: 176-183, Fasc. 2, 1958. Ill.

Loris Premuda. *Personaggi e vicende dell'ostetricia e della ginecologia nello Studio di Padova.* Padova: Attualità di Ostetricia e Ginecologia, 1958. 167 pp. Ill.

The President's Review, from The Rockefeller Foundation Annual Report, 1957. New York: 49 West 49th Street. 75 pp. Ill.

John J. Riley. *A History of the American Soft Drink Industry,* Bottled Carbonated Beverages, 1807-1957. Washington, D. C.: American Bottlers of Carbonated Beverages, 1958. xii + 302 pp. Ill.

Concepcion Soriano de la Rosa. *La Obra Quirurgica de Dionisio Daza Chacon.* Salamanca, 1958. Publicaciones del Seminario de Historia de la Medicina de la Universidad de Salamanca. Serie A, Estudios, Tomo II, Num. I, 62 pp.

Herbert Thoms. *The Doctors Jared of Connecticut.* Hamden, Connecticut: The Shoe String Press, 1958. 76 pp. Ill. $3.00. (Department of the History of Medicine, Yale University School of Medicine, Publication No. 35).

CONTRIBUTORS TO THIS NUMBER

JOHN B. BLAKE, Ph. D., is Associate Curator, Division of Medical Sciences, U. S. National Museum, Smithsonian Institution, Washington 25, D. C.

JEANNE L. BRAND, Ph. D., is Medical Research Program Specialist, Research Grants and Fellowships Branch, National Institute of Mental Health, Department of Health, Education and Welfare, Bethesda 14, Maryland.

PAUL F. CLARK, Ph. D., is Emeritus Professor of Medical Microbiology, The University of Wisconsin, Madison 6, Wisconsin.

GEORGE W. CORNER, M. D., is Historian and Visiting Investigator, Rockefeller Institute for Medical Research, New York 21, N. Y.

ROBERT J. FAULCONER, M. D., is Pathologist at DePaul Hospital, Norfolk 5, Virginia.

WALTER FEDERN, Ph. D., is an Egyptologist. His address is 105-20 66th Avenue, Forest Hills 75, New York.

CHALMERS L. GEMMILL, M. D., is Professor of Pharmacology, School of Medicine, University of Virginia, Charlottesville, Virginia.

E. M. HOLMES, JR., M. D., is Professor and Head of the Department of Community Medicine, Medical College of Virginia, Richmond, Va.

PAUL KLEMPERER, M. D., is Consulting Pathologist, Mount Sinai Hospital, Fifth Avenue and 100th Street, New York 29, New York.

THOMAS O. LAMBDIN, Ph. D., is Assistant Professor in the Oriental Seminary, The Johns Hopkins University, Baltimore 18, Maryland.

JOSHUA O. LEIBOWITZ, M. D., is a specialist in internal medicine and lecturer on the History of Medicine at Hebrew University in Jerusalem. His address is Solel Boneh Str. 7, Katamon-Jerusalem, Israel.

EDGAR F. MAUER, M. D., is a specialist in internal medicine. He is a member of the Library Committee, Los Angeles County Medical Association. His address is 1930 Wilshire Boulevard, Los Angeles 57, California.

ANTONIO PACE, Ph. D., is Professor of Romance Languages at Syracuse University, Syracuse, New York.

WALTER PAGEL, M. D., is Pathologist at Clare Hall Hospital, nr. Barnet, Herts, England. His address is: 58, Millway, London N. W. 7.

CHARLES E. ROSENBERG, is a student of the Graduate Faculty in History at Columbia University. His address is 1781 Riverside Drive, New York 34, New York.

PHILIP SAPIR, is Chief of the Research Grants and Fellowships Branch, National Institute of Mental Health, Department of Health, Education, and Welfare, Bethesda 14, Maryland.

HENRY R. VIETS, M. D., is Curator of the Boston Medical Library, 8 The Fenway, Boston 15, Massachusetts.